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Antiquity

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Editorial Notes

MANY new readers will form their first opinions (we hope favourable) of ANTIQUITY from the present number. While we are content that it should be so, we think it reasonable to point out certain things which can hardly be apparent from the contents of any single number. Some have already been hinted at in the widely distributed leaflet by means of which we increase our circulation. But there is no room there for more than the briefest and most blatant trumpetings. ANTIQUITY represents a point of view which is not otherwise articulate except in the somewhat technical articles published by learned societies—and not always there. It reports the progress of discovery and invention everywhere, and says in effect to its readers : ' These things are important and will interest you ; they are not merely the sport of specialists. What these people are doing is to unearth the basic facts of life in past ages ; in these pages you can get some idea of how they do it and how it should be done, even if you can't always master the technical processes involved. We archaeologists depend on your support and in return we try to keep you up-to-date about what is going on '.

* *

How far do the contents of this number justify such claims ? Take Major Williams-Hunt's article on Siamese earthworks. Some may say they are not interested in Siam or its history, and the writer of these notes was one such. But he is interested in earthworks, and when he was shown an air-photo of one in Siam that looked just like Badbury Rings in Dorset, he began to change his opinion. Here was a group of people who had solved the problem of home defence in the same way as our own ancestors. The facts are completely new ; no such earthworks had ever been recorded or even suspected in Siam until these air-photos revealed them, and even now no one has been to examine any single one of them on the ground. The facts are also important because they relate to a basic human need, defence. And finally their revelation, by air-photography, is a good example of ' how they do it '. Even therefore if Siamese history leaves you cold, you may still find something of interest in this article.

* *

What is the point of view which ANTIQUITY represents ? In terms of individuals, it represents the generation which between the wars built modern archaeology on the foundations laid by Haverfield and before him by Pitt-Rivers. In terms of culture—for archaeology and anthropology themselves are of course culture-traits—ANTIQUITY looks at the world past and present in the sort of way that Tylor and Morgan would probably have looked at it, had they been alive today. This is not the place for a sermon on anthropology; but we do want our readers, especially the new ones, to grasp the fact that this matter of points of view is of vital and fundamental importance, not only with regard to history and the past but in the things of everyday life. We can best illustrate ours by an example of the opposite.

⁂

A writer in the *Times Literary Supplement* (November 6, 1948), reviewing a book on New Zealand, selects as 'outstanding' a chapter on literature and the arts there. The author (says the reviewer) regards the 'basic culture of New Zealand' as 'still predominantly that which was brought over from Victorian England, and says that the greater sense of nationhood which developed during the depression among younger writers . . . has not changed markedly the attitude of the general public. He gives a warning against trying to force the growth of a national cultural consciousness'. The scene suggested here—of young writers in Victorian surroundings developing a 'sense of nationhood' during the depression, and being disappointed because everyone ignored them—has a quaint charm of its own; one can almost see the horsehair sofas and texts on the walls. But the arrogance of it! To imagine that 'national cultural consciousness' is created at all (much less 'forced') by writers young or old! We prefer to think that it is the dog that wags the tail. But this idea, that facts can be altered by words, that talking or writing at large can control or even change the culture of nations, this idea is still dominant in the world at large. It is implicit in such missionary work as consists only in preaching a gospel, though the history of all missions confutes it. (Missionaries often produce results by the example of their lives).

⁂

ANTIQUITY belongs to the school of Darwin, Tylor, Morgan and Pitt-Rivers, and regards art and literature as elements of culture to be studied as such, not as determinants thereof. We believe that this point of view can be clarified and made acceptable, and this not by preaching (and these notes must *ipso facto* be included) but by the advancement and diffusion of knowledge. We believe that all the facts of human history vindicate it. We also believe that some facts are more important than others and that we can recognize them. In archaeology—and in all other branches of science—we regard the invention of a new technique as (in outstanding examples) more important than many discoveries regarded as sensational. The discovery of a means of dating wood and other organic substances by the Carbon 14 method (as it is to be called) is regarded as far more important than that of the Hebrew Scrolls. That principle is recognized in modern affairs; the invention of broadcasting by radio is obviously of more importance than, say, the foundation of a daily newspaper. We believe in complete liberty of expression for all views and detest all inquisitions, red or black.

⁂

We think it is partly because ANTIQUITY has always represented this point of view that it is not only alive—in two senses—today, but increasing its circulation. Some readers might be surprised if they knew how closely their reactions were watched. It is they who keep us going, and it is very good for us to be thus kept on our toes. It is good for us to have competitors. Our aim is to double our circulation without lowering our standard. We might perhaps get a large circulation by abandoning all standards and becoming a mere picture paper for brainless people; but there could be no satisfaction (to the editor or his partner) in conducting such a paper, and we most certainly do not for a moment envisage it. We are doing very well at present and want nothing but the means to do better—and that will come as our circulation rises. Meanwhile, as this is the beginning of a new year, may we be excused for calling attention to the paper slip enclosed in the copies of those who have not already paid? Prompt payment is an enormous help; and payment by Banker's Order, which can begin any time, is a great saving of bother to all concerned.

* *

Publishers and authors must want to sell the books they produce, but they often themselves make this unnecessarily difficult. We receive many books—far more than we have room to review or even mention; and some of the best are published by archaeological societies. But two of the latest received had no indication anywhere of their price, and one of them gave no clue to the society's whereabouts, not even the address of the editor or secretary. How on earth can those responsible expect the public to buy their books if this essential information is withheld? We can hardly believe that such societies object to selling their special publications or *separata* (when these are made available for purchase) to non-members; for, apart from the immediate returns, that must surely help to recruit new members. Commercial publishers in this country and America usually state the price, though sometimes not on the book itself (as they should) but on an enclosed slip, being wiser in this respect than the children of light. We have observed that coyness in printing the price in the books themselves increases with the price. (The besetting sin of the commercial publisher is to omit the date or conceal it in small print behind the title-page—which is its proper place.) The information which an interested reader may be expected to want is the address to which he or his bookseller should write (1) to obtain a copy (2) to become a member, and the cost of each. Even when this information is given, it is often buried in an elaborate list of the society's officers, and he is not helped in deciding whether he should write to the editor at his private address (when given, sometimes only in the list of members in another fascicule) or to the society's headquarters, or to its secretary or to the printer. By this failure to observe the elementary rules of business, learned societies impose unnecessary handicaps upon themselves.

* *

Mr Collins writes:—It is kind of you to devote so much space to our Blewburton excavations. Your admirable summary, however, contains one slip to which attention should be called: in your last paragraph but one you attribute the burial in the ditch to Iron Age A. If this were so it would upset the otherwise established sequence of pre-earthwork A2 followed by post-earthwork AB. In fact, the pot found with the burial shows B influence in its profile and in its incipient bead-rim. The adze is of the type found on sites such as Hunsbury and Glastonbury, where B influence is very strong.

* *

The Editor wishes to thank all those readers who have written to congratulate him on the honour conferred on him by the King. He hopes to be able to write and thank each person individually; but as he left England for the Sudan almost immediately afterwards, many such letters could not be answered before his departure.

Cave Men's Buildings

by V. Gordon Childe

THE popular epithet for our pleistocene forerunners is still 'cave men'. It gives the impression that they were as completely dependent on Nature's bounty for shelter as for food. And of course that impression was originally intended by archaeologists themselves. Most remains of middle and upper palaeolithic cultures were, and still are being, collected from caves. Other sites that yielded similar relics have been designated 'open stations', with the inevitable, if not deliberate, implication of the absence of any shelter. Yet from many caves there have long been known engravings or paintings that seem to represent tents or more substantial structures and that have been designated 'tectiforms' for this very reason. Not all prehistorians accept this obvious interpretation, and few of those who do have realized its significance. Perhaps they were waiting with proper scientific caution for monumental evidence.

Such evidence is now available in plenty. But as it is mostly published in Russian, and badly at that, it has not been at all widely appreciated. Yet the construction of dwellings twenty thousand or more years ago is so striking a fact in cultural history as to justify a summary of the rather inaccessible accounts that have appeared.

Physical remains of the simple tent-like structures inferred from the commoner tectiforms have been identified at Gagarino and Kostienki on the Don in the Ukraine and at Mal'ta and Buryet on the Angara in Siberia. At Gagarino (1) the 'hut foundation' was an irregular saucer-shaped excavation in the löss, nowhere more than .50 m. deep. and measuring some 5.50 by 4.50 m. across. A silo trench, dug by peasants, that had originally revealed the archaeological deposit, had cut right through its centre along the major axis thus presumably destroying the hearth and the entrance. Little evidence for the superstructure was left, but Zamyatnin noticed large slabs of stone and mammoth tusks lying near the margins of the depression. This dwelling was occupied by Gravettian mammoth-hunters as was a similar one subsequently discovered at Kostienki (2) below the large 'communal house' to be discussed later.

The dwelling at Mal'ta (3), a site celebrated for statuettes of mammoth ivory, was essentially similar. The oval depression in the original soil was bordered with stone slabs. The hearth, in the centre of the floor, was a shallow basin with a stone slab at the bottom and slabs of stone on edge at either side. The whole was full of charcoal and bone-ash. At Buryet (4) between 1936 and 1940 four analogous foundations were uncovered. All belong to the same 'period' (a phase of the Upper Palaeolithic which cannot as yet be defined more closely either archaeologically or geologically) which does not of course necessarily mean that all were occupied in the same year. A comparatively

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[1] Zamyatnin, *La Station aurignacienne de Gagarino*, Moskva, 1934.

[2] Efimenko, *Pervobytnoe Obshchestvo*, Leningrad, 1938, p. 383, n. 1.

[3] *Paleolit S.S.S.R.* (*Izvestiya* GAIMK., 118), Leningrad, 1935, pp. 116–7.

[4] *Kratkie Soobshchenniya* . . . IIMK, x, 1941, pp. 16–19.

full description of the foundations has been published together with a detailed plan of one, but no sections.

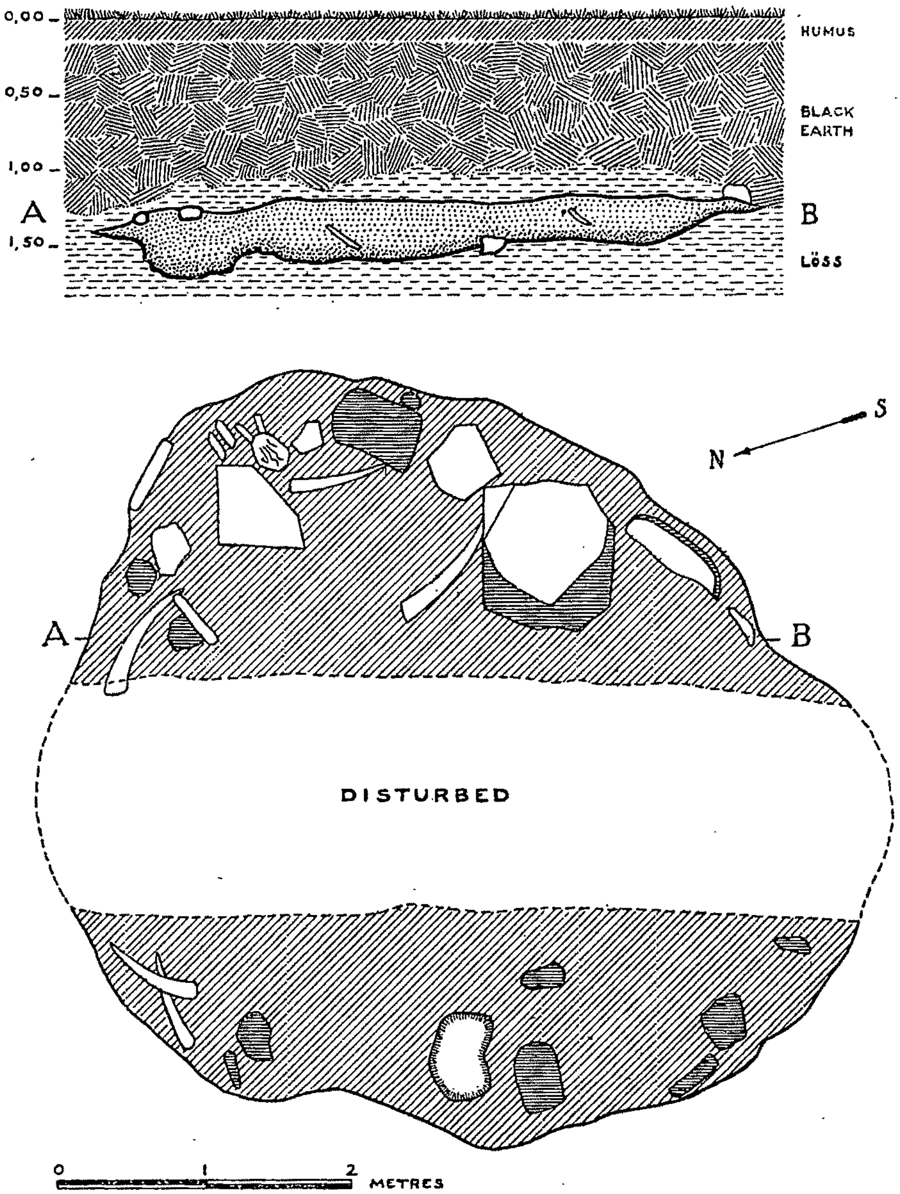

Fig. 1. PLAN AND SECTION OF HUT-FOUNDATION AT GAGARINO
after Zamyatnin

As elsewhere, the edge of the oval depression was delimited by stone slabs, measuring on an average 1.0 by 0.7 m. across, that presumably once stood on edge, and by large bones such as rhinoceros skulls. Just within the margins thus defined lay phalange bones of mammoth, often under prostrate slabs but spaced at regular intervals round the depression. An extra pair, further from the centre than the rest, flanked what is supposed to have been the entrance. Okladnikov supposes that these bones helped to support the roof but not as pillars so much as struts upon the tops of which would have been supported inward-slanting poles the lower ends of which would have rested on the ground where

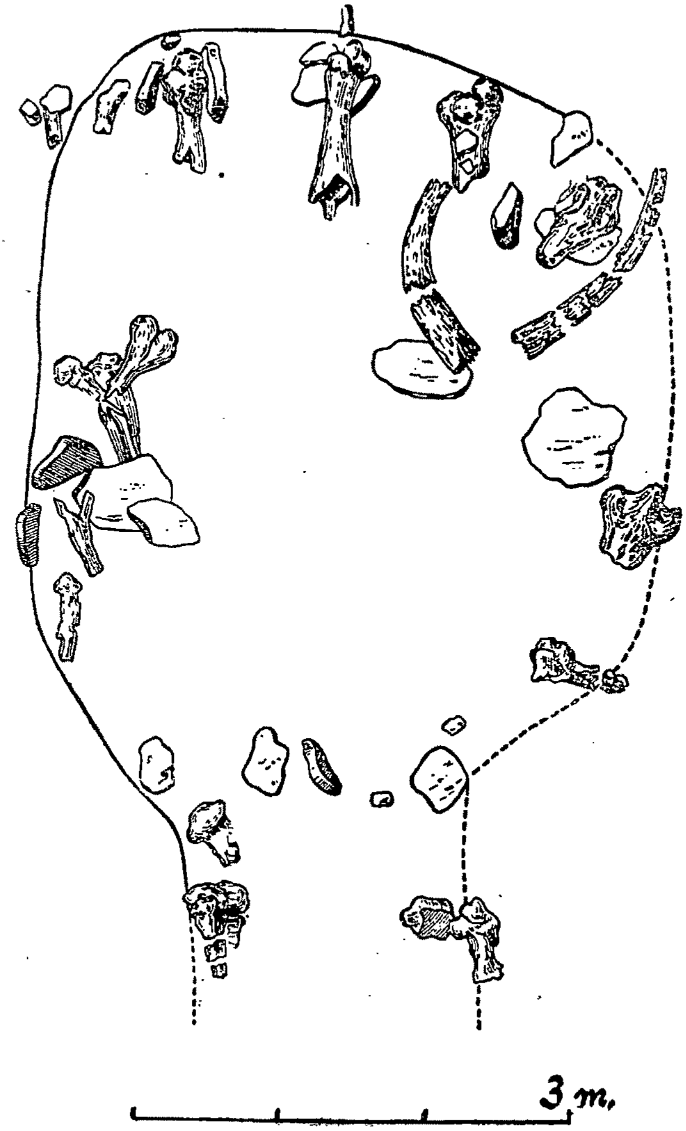

FIG. 2. PLAN OF HUT AT BURYET

the slabs stood along the margins of the depression. On the latter's floor were found numerous reindoor antlers, sometimes interlocked. These, the excavator suggests, were fallen from the roof where they had once formed a sort of lattice between the converging poles to serve as a frame supporting a covering of skins.

Of course traces of such small and probably temporary shelters are not altogether unknown in Western Europe (5). More novel is the Russians' claim to have discovered remains of large communal houses which would have served as permanent winter quarters for larger social groups. The first observations justifying this claim were made at the village of Kostienki (6) on a site previously identified by Polyakov and known already in the 18th century. When Efimenko began excavation here in 1923 he soon recognized that the bulk of the relics were concentrated in a hollow in the old löss surface which eventually proved to be an irregular oval, 35 m. long by some 15 m. wide. Along its major axis was a row of nine (or perhaps eleven) hearths spaced at intervals of two metres. All complete artifacts lay on the bottom of this depression. But around its margins and above its floor broken artifacts and bones were recovered. Efimenko ingeniously inferred that they had once been incorporated in earth that had been heaped up round and over a roof of perishable material that had originally covered the whole depression. The latter would accordingly represent the floor of a single communal dwelling wherein nine (or eleven) ' natural families ' lived under a single roof.

For the superstructure of this communal dwelling no evidence has been published save the mention of the usual large mammoth bones round the margins of the hollow and of various holes in its ' floor '. Some holes were admittedly small storage pits, but others may well have been sockets for posts. Unfortunately no plan whatsoever of the site has yet been published. Its interpretation as a communal dwelling comparable to the long houses of the Iroquois or the Dayaks is certainly plausible and attractive. But the difficulties of roofing a dwelling 15 or more metres wide on a treeless tundra, where wood was so scarce that according to Efimenko bones were habitually used for fuel, are obviously formidable. Efimenko's account accordingly needs confirmation by accurate plans and sections.

Yet some support for it may be found in the fuller description of the contemporary settlement of Pushkari on the Desna near Chernigov (7). Here the ' house ' was again an irregular oval depression, but this time only 12 m. long and 4 m. wide and containing only three hearths, once more in a row down the major axis. No sections have been published, but, it is stated, the edges of the depression were gently sloping for some 8 m. along the north-east side and on the opposite side between the western and the middle hearths ; elsewhere they were steep and actually vertical in the southeast and northwest corners. In the floor besides the hearths were some thirty holes, mostly in the area around the middle hearth, 10 to 12 cm. across and some 10 cm. deep. Their filling, red in colour, comprised flints, ashes and bones, and so Boriskovskiĭ regards them as storage pits. He does not mention the attractive hypothesis that some may have been post-holes, and the plan does not show whether they would have contained structurally significant posts. Four mammoth jaws, found in vertical positions near the margins of the hollow, are, however, supposed to have played some part in roofing the area. In any case the inhabitants of Pushkari would have formed a very small clan.

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[5] Lang-Mannersdorf (*Mannus*, XIII, 76) ; Fourneau du Diable (*Mem.* 10, 1932, Institut de Paléontologie humaine).

[6] *Trans. 2nd Internat. Conf. Assoc. for Study of Quaternary Period in Europe*, vol. v, Leningrad, 1935, pp. 93–4 (German) ; Efimenko, *Pervobytnoe Obshchestvo*, pp. 447–54.

[7] *Kratkie Soobshchenniya*, VII, 1940, pp. 81–6.

Foundations for two much longer and more capacious houses have been reported from a second site at Kostienki, Kostienki IV (8), but these belong to a later period than Kostienki I, Gagarino or Pushkari, comparable probably to the early Magdalenian of western Europe. Only an outline plan without sections and a preliminary description have been published. House A was represented by a hollow, 34 m. long and 5.5 m. wide, along the major axis of which were nine hearths in a row. Low ridges, rising above the general level of the hollow's bottom, seem to have divided the house into three compartments—I, II and III on the plan. In addition to the hearths, many deeper hollows—20 to 35 cm. across and 30 to 40 cm. deep—had been dug in the floor; the lower part of their fillings was coloured bright red and consisted largely of hare bones. Rogachev regards them as storage pits; an explanation as post-holes is again tempting, attribution to burrowing animals not excluded.

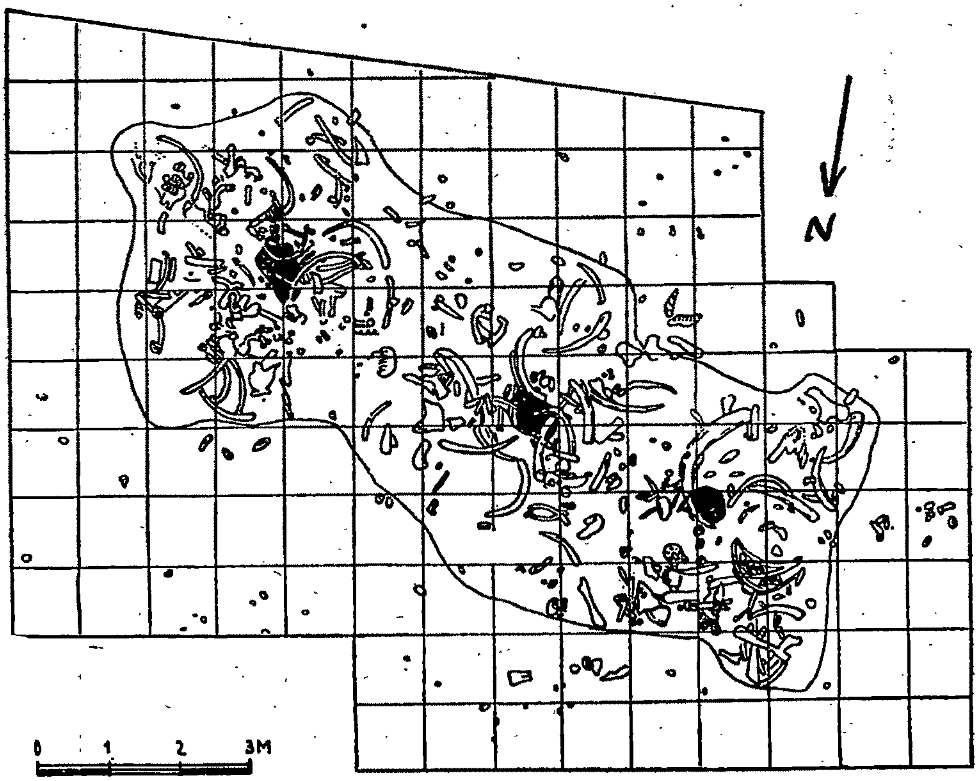

FIG. 3. PLAN OF GRAVETTIAN SETTLEMENT AT PUSHKARI
The irregular enclosing line marks the edge of the excavated habitation
after Boriskovskiĭ

House B lay 15 to 20 metres from the first and consists again of a long hollow (IV on plan), 21 m. long by 5.5 m. wide. Nine hearths—saucer-like depressions some 60 to 70 cm. in diameter and 8 to 12 cm. deep filled with ash—were strung out along the

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[8] ibid, IV, 1940, 36–8.

major axis while smaller holes had been dug in the floor as in A. The long house is cut by two round hollows (V and VI) each about 6 m. in diameter and containing a single central hearth. While the faunal remains were still explicitly pleistocene, the flint and bone tools recovered from these round hollows differed from those in the long houses. They in fact must represent the foundations of two simple round huts like those of Gagarino and Kostienki I that had been dug into the older long house B. In rejecting this view Rogachev writes ' The partial covering by the deposit in VI of the deposit in IV might be the result of slip from VI which lies a little higher up the slop '. But ignoring these later intrusions, Kostienki IV would show us a settlement of two clans comprising 18 single families.

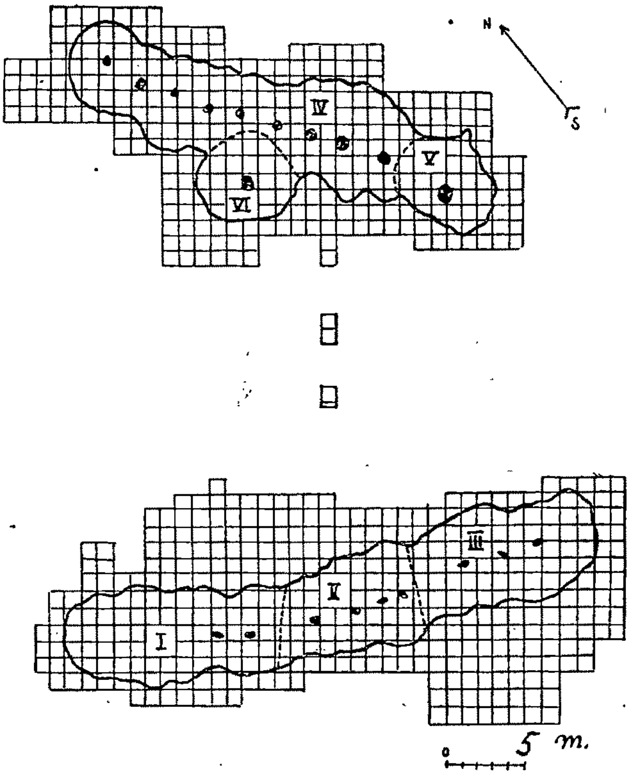

FIG. 4. PLAN OF THE LATE PALAEOLITHIC SETTLEMENT KOSTIENKI IV

The excavated areas are divided up into one-metre squares; the irregular lines mark the limits of the two excavated 'houses'; the broken lines, low ridges dividing the excavated area into 'rooms', numbered I to VI; the black spots mark hearths

after Rogachev

An even more complex settlement dating from a rather later phase of the Old Stone Age has been described, again with very inadequate plans, by the late Prof. Gorodtsov (9)

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[9] ' Sotsialno-ekonomicheskiĭ stroĭ drevnikh obitateleĭ timonovskoĭ paleoliticheskoĭ stoyanki ', *Sovietskaya Etnografia*, 1935 (3), pp. 1–7; cf., Hančar in *Quartär*, IV, 1942 (German).

at Timonovka also on the Desna. Between 1929 and 1934 he had uncovered six long houses, four large storage pits, three open-air hearths and two working places. The houses were rectangular excavations, 6 to 12 m. long, 3.5 m. wide and 2.5 or 3 m. deep. Traces of timber revetments to the vertical walls are reported, and a flat roof covered with earth and rubbish is assumed. Access was provided by sloping ramps about one metre wide. Hearths—again hollows 1.10 m. in diameter and .6 m. deep—were observed only in houses I and VI. Houses I and VI seem to be paired with the unheated houses II and VII. (The incomplete house V may have formed a similar pair with IV). The working places of flint-knappers are conspicuously connected with the unheated houses IV and VII while storage pits seem to be connected with houses IV, VI and VII. Gorodtsov further remarked that needles and other tools proper to women were found only in the houses containing hearths while the larger storage pits were attached to these also. From these observations he drew highly illuminating albeit speculative sociological deductions which cannot be discussed here.

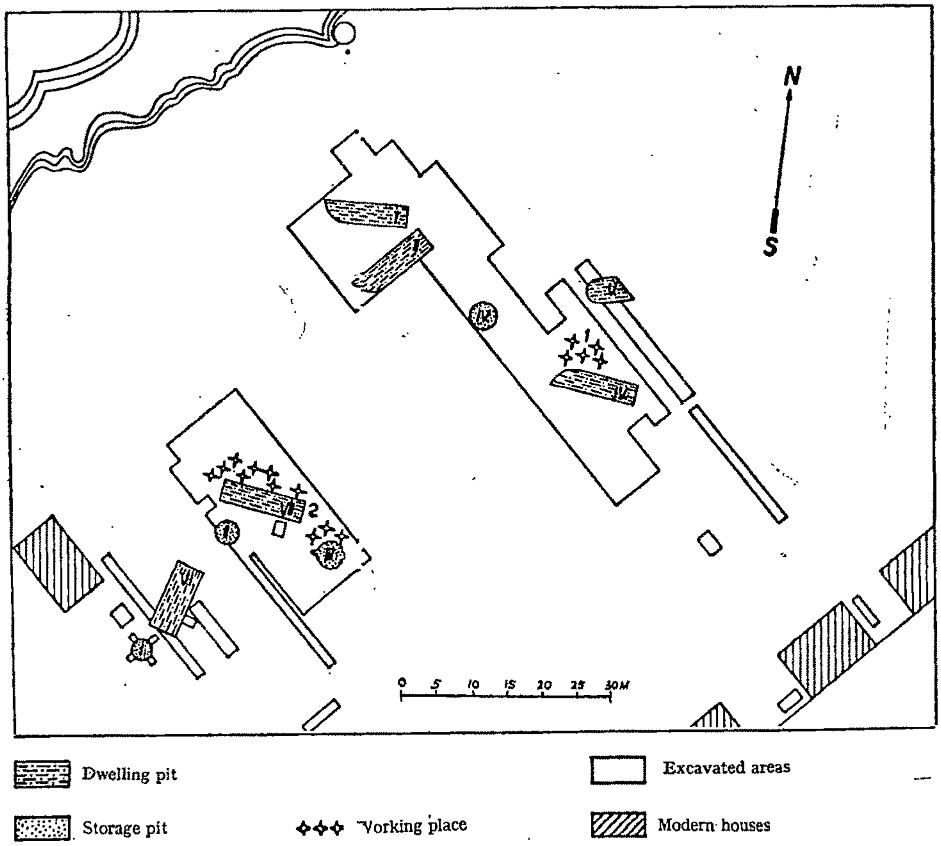

FIG. 5. PLAN OF THE EXCAVATIONS ON THE LATE PALAEOLITHIC SITE OF TIMONOVKA
after Gorodtsov

Soviet archaeologists may be justly proud of having demonstrated such an immense prolongation of the history of domestic architecture. But they have not yet presented adequately the content of this new volume of architectural prehistory. Its correct evaluation demands accounts of Kostienki, Pushkari and Timonovka as detailed and as well illustrated as the classic monographs on Glastonbury or Köln-Lindental. In 1938 Efimenko announced that a final report on Kostienki I was in preparation. The war intervened, but Efimenko survived it (rumours of his ' liquidation ' are false), so the work may still be expected. Let us hope it will be as complete and as lavishly produced as German accounts of neolithic villages.

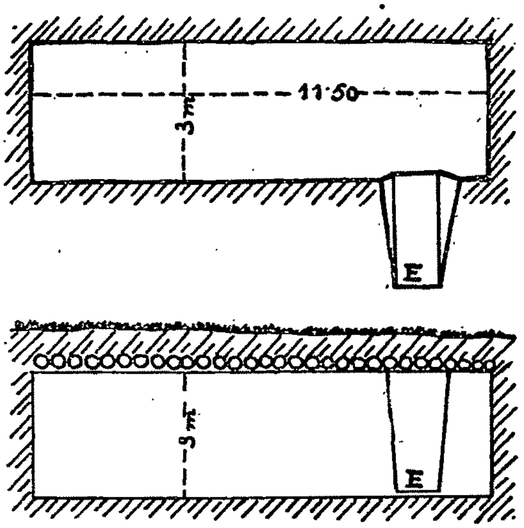

FIG. 6. PLAN (above) AND LONGITUDINAL SECTION (below) OF A HABITATION-PIT IN THE LATE PALAEOLITHIC SETTLEMENT OF TIMONOVKA

after Gorodtsov

In the meantime it may be relevant to note in conclusion that last year Rust reported a ' house ' of the Hamburgian culture. A ring of big stones is taken as marking the outlines of a tent which in turn stood in a larger, horse-shoe shaped enclosure, 9.5 m. by 10 m. at the open end of which, outside the ' tent ', was the hollowed hearth with a working place beside it. This presumably represents only a summer encampment, but again only a preliminary report (10) is available.

NOTE.—We feel that an apology is needed for publishing these atrocious plans in ANTIQUITY, which has always aimed at a high standard of cartography. But they are the only ones available of the only known ' cave men's buildings ', and neither we nor the author of the article had any alternative.—ED.

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[10] *Hammaburg*, I, 1948, pp. 34–6.

Archaeological Discoveries on the Blue Nile

by F. Addison

IN a recent article in this journal (1) Mr O. G. S. Crawford referred to the people of the Sudan as a people without a history, and he drew attention to the possibilities of filling some of the lacunae in their history by the excavation of sites south of Khartoum especially along the White Nile. It is, of course, true, as the responsible authorities are well aware, that a good deal of exploration remains to be done in the Sudan and awaits only the necessary funds. It is probable, also, that many ancient village sites in the area between the Blue and White Niles now lie beneath the cotton fields of the Gezira Irrigation Scheme. It is therefore the more unfortunate that some archaeological discoveries which *have* been made have either been inadequately published or their publication has been inordinately delayed.

In this latter respect archaeology has been singularly unfortunate in the district round Sennar, on the Blue Nile, a region which appears to have been important throughout the history of the Sudan. It is doubtful if the Egyptians of the Old Kingdom penetrated as far south as this, though their descendants may have done so in New Kingdom times ; but the area was certainly a centre of civilization in the heyday of the Meroitic kingdom, about the beginning of the Christian era, and, nearer our own times, it was the headquarters of the powerful Fung kings of the 16th to 18th centuries.

The name Sennar has, within the last hundred years, been given to three different towns (see map, FIG 2). The first of these was the site of the Fung capital and this, now in ruins, was replaced in the early days of the present administration by a new town built a little to the south. This was then called Sennar and the ruined town Old Sennar. Still further upstream lay the hamlet of Makwar, close to the line of the projected dam, and this latter, during its construction, was known as the Makwar Dam. When it was completed, however, it was officially named the Sennar Dam, and Makwar, now swollen to a town, was re-named Sennar. The town previously known as Sennar was now called Old Sennar, later changed to Sennar Junction, and the ruined Fung site was left nameless. These changes are somewhat confusing, and it is as well to bear in mind that the Sennar described by the European travellers of the 16th to 19th centuries is not the same as that which appears upon the most recent maps.

The discoveries with which it is proposed to deal in this account are not new ; they have been, or are being, published elsewhere, but it has been suggested to me that the available information might with advantage be collected within the compass of a single article. The first of them was made as long ago as 1914 as a result of planned excavation on a site on the right bank of the Blue Nile (No. 1 on the map, FIG. 2) near the village of Abu Geili. This was one of the sites selected for excavation by the late Sir Henry Wellcome and the actual operations were in charge of Mr Crawford. A report on this excavation, written jointly by Mr Crawford and myself, is now in course of publication under the auspices of the Wellcome Trustees.

The next discovery was accidental and was made towards the end of 1921 on the right (east) bank of the river during the excavation of a railway cutting incidental to the

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[1] ANTIQUITY, vol. XXII, no. 85, March 1948.

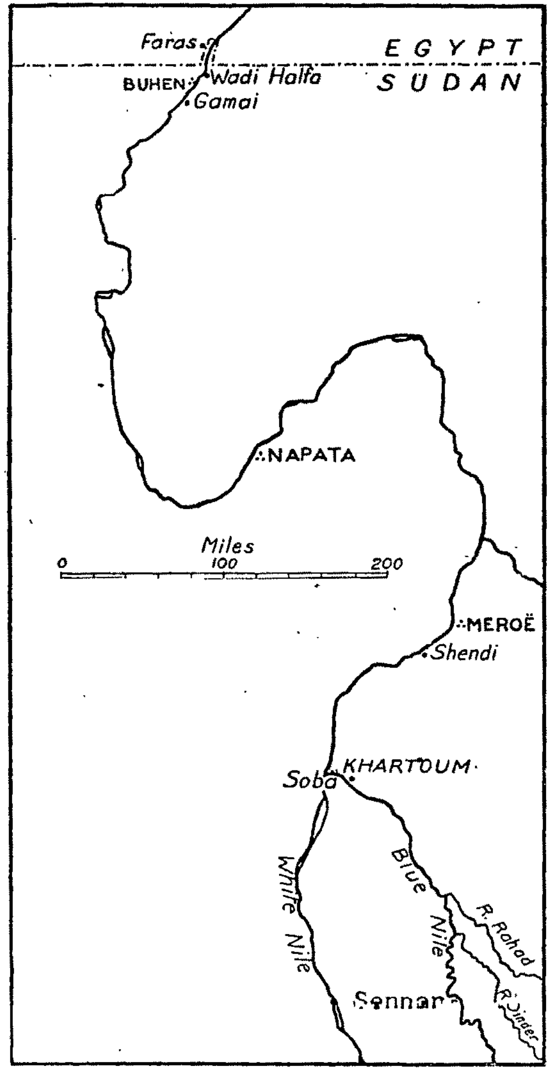

Fig. 1. THE NILE FROM SENNAR TO FARAS

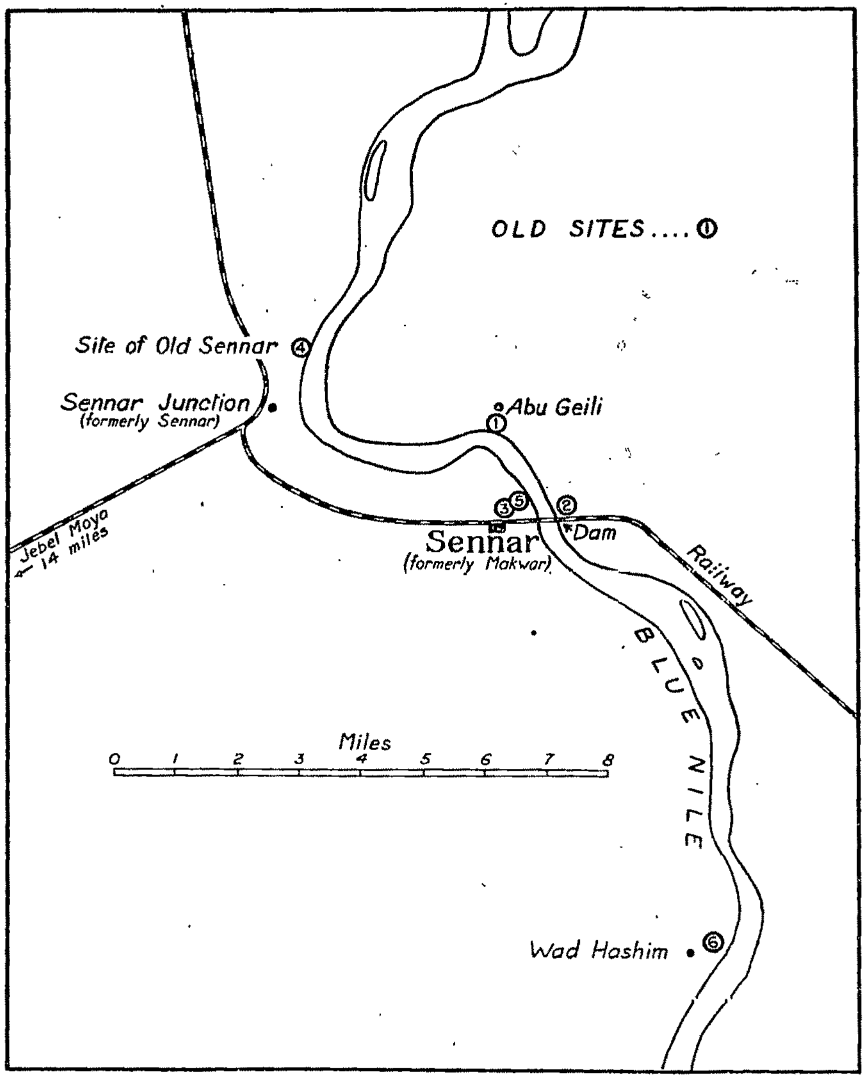

Fig. 2. THE OLD SITES IN THE SENNAR DISTRICT

construction of the Sennar Dam (Site 2 on map, FIG. 2). The cutting was a short distance from the edge of the river, a little downstream of the line of the dam, and it cut through an ancient cemetery of which the existence had previously been unknown. It was some time before news of this happening filtered through to the Antiquities Service and, when I visited the site in March 1922, I found that all the disturbed graves, an unknown number, had been completely plundered by the labourers and others engaged on the work. The spoil from the cutting had been dumped on its eastern side and there were stacks of bricks and other materials on the west, so that the extent of the cemetery was, and still remains, uncertain. A small collection of objects from the graves had, however, fortunately been preserved by one of the officials on the site, and these were handed over to me for the Sudan Government Museum. They seemed of sufficient importance to warrant publication in *Sudan Notes and Records*, the usual vehicle for contributions from officials of the Sudan Government, and I was accordingly instructed to prepare an account for this purpose. Before it was published a copy, with photographs of the objects, was sent, as a matter of courtesy, to Sir Henry Wellcome, who still maintained his excavation concession although he had not worked it for eight years. Sir Henry, somewhat surprisingly, took the view that the disturbed cemetery lay actually within the area of his concession, and he not only strongly objected to the proposed publication but demanded that the objects described should be sent to England to be stored with the as yet unpublished material from his own excavations. Whether or not his claim was justifiable need not concern us here; the Sudan owed much to Sir Henry's benefactions in the past, and the Governor-General at the time decided that the matter was not worth arguing about. The account of the discovery was accordingly suppressed and the few objects which had survived the looting of the cemetery were packed up and consigned to Sir Henry's warehouse in England. They never reached their destination; the ship carrying them struck a reef in the Red Sea and sank with all her cargo! Fortunately, that was not quite the end of the story as will shortly appear.

A further accidental discovery was made near the Sennar Dam in June 1925, this time on the left (west) bank of the river and downstream of the dam, when a grave was encountered during the digging of an irrigation channel to supply water to the merkaz garden. (Site 3 on the map, FIG. 2). Once again immediate looting took place, regrettably connived at on this occasion by an English contractor; but the local Assistant District Commissioner took action as soon as he heard of the occurrence and he was able to recover part of the contents of the grave consisting of a number of pots and a few beads. I was myself in England at the time but the objects were kept in official custody until I was able to see them later in the year. I made a few notes, took some photographs of the pots, and then handed them over to Sir Henry Wellcome's representative who bore them away to the oblivion of his camp at Jebel Moya.

The first rift in the hitherto impenetrable veil which had hidden the archaeology of the Sennar district appeared in 1934. In that year Mr A. J. Arkell submitted to *Sudan Notes and Records* an account (2) of three ancient burials exposed by erosion which he had observed whilst serving as District Commissioner, Sennar. This the editorial committee of the journal published without further ado, and, as it called forth no protest from Sir Henry Wellcome on its appearance, they decided to print my note on the earlier discovery which had been suppressed some years before. It was duly released in 1935(3), with modifications made necessary by the fact (not stated at the time) that the objects

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[2] *Three Burials in Sennar District*, by A. J. Arkell. *S.N. & R.*, XVII, 1934, p. 103.

[3] *Antiquities at Sennar*, by F. Addison. *S.N. & R.*, XVIII, 1935, p. 288.

described had for a long time been lying at the bottom of the sea. It also included some mention of the discovery of the grave on the west bank of the Blue Nile in 1925.

Coming now to the details of these various discoveries, it would not be proper to anticipate the report on the excavation at Abu Geili further than to say that it deals with a village site and a cemetery, close to each other but separated by nearly a thousand years in time. The village was occupied during the early centuries of our era while the cemetery belongs to the Fung period. The village site yielded masses of broken domestic pottery, beads, and other debris of occupation, but there was nothing to suggest a standard of life higher than that to be expected in a native village.

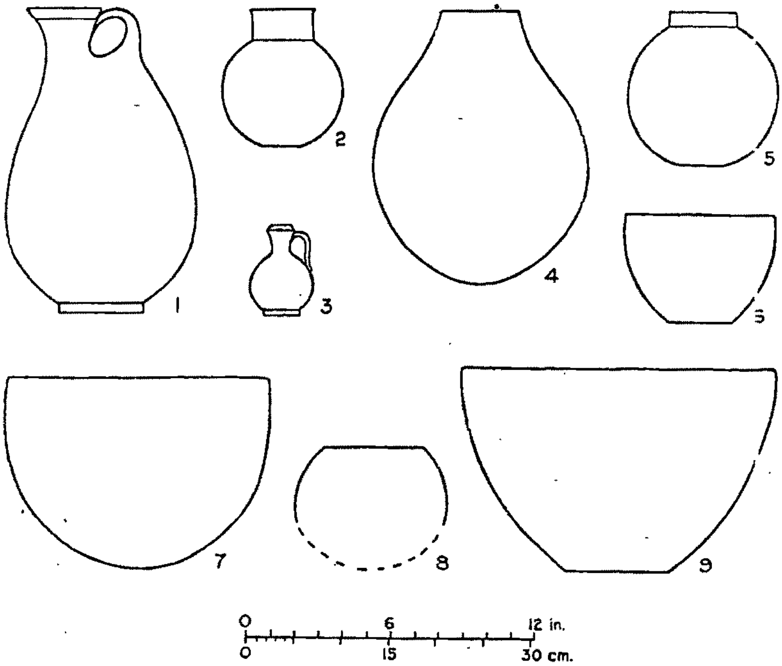

FIG. 3. POTTERY FROM THE MEROITIC CEMETERY (Site 2)

A much higher degree of material civilization was indicated by the objects from the cemetery disturbed in 1921 by the building of the dam (Site 2). They consisted of beads and other small objects, pottery and bronze vessels, and the collection was such as to be dated with some confidence to a period covered by the last century B.C. and the first century A.D. The beads were of carnelian, quartzite, Lydian stone, faience and glass; there were pendants of clear quartz, a faience Bes figure, a faience figure of the Amon ram with sun disc, an amulet of mottled granite in the form of a frog and another of crystal (? glass) in the form of a fish. There was also a mud impression from a signet ring which may have been looted at the time the graves were disturbed.

The pottery has already been described, with photographs, in *Sudan Notes and Records*, but outlines of most of the forms are given here in FIG. 3; we are not immediately concerned with the decoration upon them. Nos. 1, 2 and 3 were wheel-made, of red ware, and were in all probability imported from the north. Vessels similar to the handled jar or *askos* No. 1 were found in 1908–10 by Woolley and MacIver at Buhen (**4**), in the extreme north of the Sudan, and by Reisner in the western cemetery at Meroë. Small jugs such as No. 3 were also found at Buhen and by Griffith in the Meroitic cemetery at Faras (**5**), even farther north. The jar No. 2 is another common Meroitic form. The remaining pots were hand-made and can safely be regarded as local products. The only one which calls for comment is the jar No. 4, a distinctive type which seems to have had much the same distribution as the wheel-made types but as a result of a movement in the opposite direction. It was found at Gamai (**6**), Faras (**7**) and Karanog (**8**) (in Egypt north of Faras and outside the boundary of the map, FIG 1), and on those sites was usually dated to the 1st–2nd centuries A.D. Yet pots of this type were found at Jebel Moya (**9**) in a context which suggests that they were some centuries earlier in date than those found further north. The form seems to have originated in the central Sudan, possibly in Kordofan, and its presence in the extreme north of the country is to be explained by a movement of trade or of people from south to north. It may be observed in passing that flat-bottomed bowls such as No. 6 in FIG. 3 were also found at Jebel Moya.

More important than the pottery and small objects were the bronze vessels, which occurred in a profusion unknown elsewhere in the Sudan. Photographs of the various types have already appeared (**10**) but outline drawings are again given in FIG. 4. There were ten examples of type 1, two of type 2, three of type 3, three of type 4, one of type 5 and the remains of three large basins of type 6. These latter all had the same kind of decorated rim, but one had handles of type 7 while the other two were furnished with handles of type 8. The smaller vessels were in quite a good state of preservation but the large basins, of thin metal in comparison with their size, were broken and badly corroded. It is to be remembered that the objects enumerated were only those salved from the looting of the cemetery; the total originally buried must have been much greater.

There is nothing remarkable about the hemispherical bowls of type 1; they have been found on every Meroitic site on which excavations have been made, and one or two were found even at Abu Geili. The deeper form, type 2, is a survival from an earlier period and was not common in Meroitic times, while the straight-sided type 3 is unusual with no exact parallel in the north. The carinated bowl 4 is a characteristic Meroitic type, examples having been found at Meroë (see below) and on northern sites. The jug 5 is similar to one found by Garstang on the Meroë city site in 1909–14. The large basins of type 6 are, as far as I know, unique in the Sudan, for I am unaware that any others have been found elsewhere in that country. These things must have been an expensive

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[4] *Buhen*, by C. L. Woolley and D. Randall MacIver. (*The Eckley B. Coxe Junior Expedition to Nubia*, vol. VIII).

[5] *Oxford Excavations in Nubia*, by F. Ll. Griffith. *Annals of Archaeology and Anthropology* (Liverpool), vol. XI.

[6] *Excavations at Gammai*, by Oric Bates and Dows Dunham. (*Harvard African Studies*, vol. VIII), p. 41 and pl. LXII, 27.

[7] loc. cit., pl. XLII.

[8] *Karanog, The Romano-Nubian Cemetery*, by C. L. Woolley and D. Randall MacIver. (*The Eckley B. Coxe Junior Expedition to Nubia*, vol. IV), pl. 102.

[9] *Jebel Moya*, by F. Addison (*The Wellcome Excavations in the Sudan*), pls. CXI, CXIII. A discussion of this type of pot is given on p. 220.

[10] loc. cit.

imported luxury at the time and it is remarkable that they should have been found so far from the Mediterranean.

All the objects so far mentioned are, as already explained, now for all practical purposes non-existent, but there are others from the same cemetery which still survive. When I was entrusted with the task of publishing the results of Sir Henry Wellcome's excavations in the Sudan I found, during the course of my examination of the material and records, certain objects labelled ' Makwar ' which had obviously come from the cemetery under discussion. I discovered that Sir Henry, stimulated doubtless by my note and photographs sent to him in 1922, had immediately instructed his representative in the Sudan, Major J. S. Uribe, to make further investigations. Major Uribe accordingly took some labourers and a sifting machine to the spot and set to work. The cemetery had by that time been so completely plundered that his labours were not richly rewarded, but he recovered a few faience ball and cylinder beads, some pendants and some fragments of bronze vessels. The pendants, of carnelian, quartzite and clear quartz, are illustrated in the PLATE, b. They are similar to those previously recovered and lost again and are of the same types as those found in the Meroitic cemetery at Faras (11). The bronze fragments included some rim pieces from a large basin of type 6 above, and the remainder were of a hemispherical and a carinated bowl respectively. This latter was kindly cleaned and repaired by Dr H. J. Plenderleith and it is shown in the PLATE, a. Below

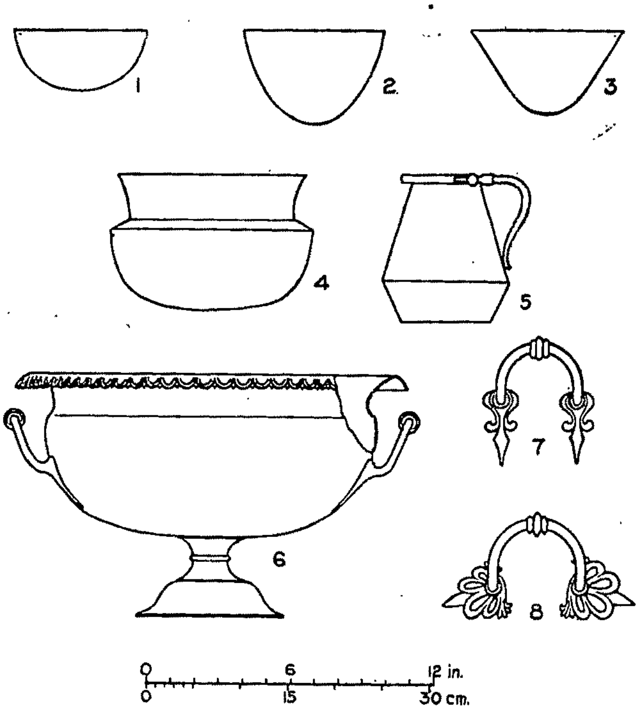

FIG. 4. BRONZE VESSELS FROM THE MEROITIC CEMETERY (Site 2)

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[11] *Annals of Archaeology and Anthropology*, vol. XI, pls. LXII, LXIII, LXX.

the rim, on the damaged side not shown in the photograph, are two incised hieroglyphs as shown in FIG. 5. Being unable myself to read any meaning into this inscription, and knowing that bowls of this type had been found during Reisner's excavation of the Meroë pyramid fields, I sent a copy of the inscription and a sketch of the bowl to Mr Dows Dunham. He himself conducted much of the excavation and is engaged on the publication of the results. He replied as follows:

> 'Your bronze bowl from a site near the Sennar dam is paralleled in our excavations at Meroë (West Cemetery) in six tombs, all of which lie within the date range 100 B.C.–A.D. 100, and they are recorded *only* there by our expedition. They were not found by us either at Kurru or Nuri, and thus appear to be limited to the Middle Meroitic period as you suggested. This seems to be in general agreement with Petrie's dating of the form, see *Funerary Furniture and Stone Vessels*, Pl. XL, nos. 58 and 62–6, which he considers to be " Greek or Roman ".
>
> The inscription in Egyptian hieroglyphs presents a problem. I know nothing resembling it from our excavations. The group *Hnm.wcnh* is, it is true, listed by Ranke in *Personennamen* (275 : 8) as occurring twice in the O.K. and once in the M.K., but not later, so there is no evidence to show that it is here a personal name, for the late period is clearly involved. *Wtb.* III, p. 378–9 gives us, I think, something possibly suggestive. Here VII (a) is the name of the funerary temple of Tuthmosis I, VII (b) is given as meaning ' Necropolis ' in the Late period, and there is also a Greek example with the meaning Hathor-Isis. All this suggests that the group has some relation to a funerary designation of the vessel.
>
> While there is hardly enough evidence to prove anything definite, I am tempted to suggest that this vessel may well have been an import from Egypt in Late Ptolemaic or Roman times, the inscription having been incised by an Egyptian craftsman to mark it as destined for funerary use. I feel that the presence of this inscription makes it unlikely that the vessel is of local Meroitic manufacture, and we have plenty of evidence from Meroë itself indicating the fairly common import of bronzes and other objects from Egypt'.

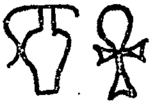

FIG. 5. INSCRIPTION ON BRONZE BOWL, 1:1

The presence of an inscription on the bowl recovered by Major Uribe inevitably suggests the possibility that other bowls from the cemetery may have been similarly inscribed. Those now lying in the Red Sea were apparently devoid of inscription or decoration, but they were not cleaned before being sent away and the question as to whether they were inscribed or not must for ever remain unanswered.

The remains of the town or settlement to which the cemetery belonged have not been found. Abu Geili is perhaps too far away, and in any case the objects found on the habitation site there are not comparable with those described above; even the pottery is different. The carinated bronze bowls found at Meroë occurred in the graves of the higher aristocracy, hence it is likely that bowls such as that illustrated in the PLATE belonged to persons of like estate. Obviously there must have existed on the east bank of the Blue Nile a town which for a time was comparable in importance with Faras in the north and with Meroë itself. Such a town would have had easy communications with Meroë without the necessity of crossing the river. (The beds of the rivers Rahad and Dinder are dry during the winter months.) Quite possibly it was the administrative centre of the southern provinces of the kingdom, the seat of a governor who may even have been a scion of the royal house itself. While it existed there was, on the evidence of the finds, freedom of communication not only from Sennar to Faras but from Sennar to the Mediterranean, and this connotes good administration and settled conditions in the Nile valley which were not to recur for two thousand years. This southern capital was abandoned—or possibly it may have been attacked and overwhelmed—some time towards the end of the 2nd century A.D., when the Meroitic kingdom was beginning to break up from its perimeter. It does not seem ever to have been re-occupied but some trace of it may yet be found, unless, indeed, it is buried for good beneath the dam.

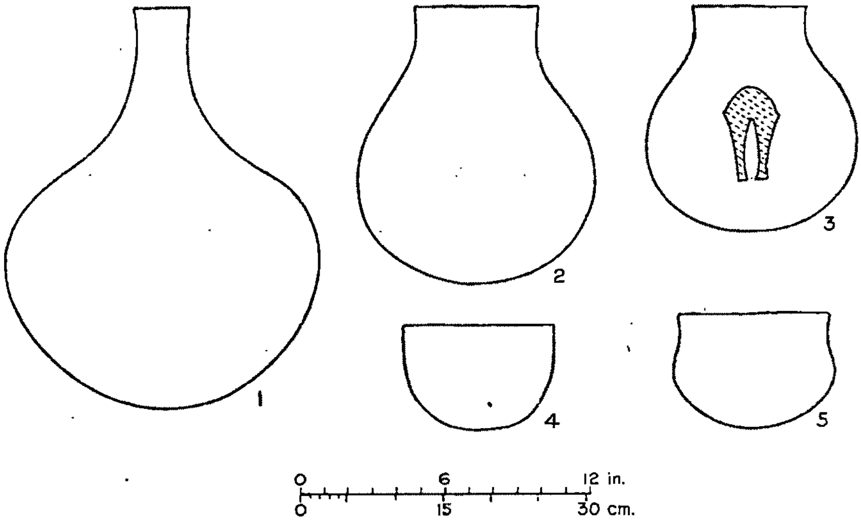

FIG. 6. POTTERY FROM SITE 3, SENNAR

The grave discovered in 1925 near the dam on the West bank of the river (Site 3) belonged to a much later period. According to the report of Mr G. M. Hancock, the Assistant District Commissioner at the time, it was found about 40 cm. below ground level, was circular, about 2 metres in diameter, and was approached from the northeast by a sloping passage, the entrance to the tomb being closed by a stone slab. It was said to have originally contained 'about 40 pots, a number of disintegrated human bones, some scraps of silk and some coloured beads'. As regards the pottery Mr Hancock wrote: 'Unfortunately most of the large *burmas* and *gullas* were stolen by workmen on the dam before I could secure them. However, I now have two *burmas* and about twenty smaller pots which apparently did not appeal to the thieves to the same extent as the larger ones'.

The pottery forms are shown in FIG. 6. The larger vessels (presumably the *burmas*) are those shown at 1, 2 and 3, and the small bowls varied in shape from No. 4 to No. 5; all of them were hand-made. The bowls, except one which was red-polished, were of black ware, smoothed but not polished. They were devoid of decoration on the outside but most of them had a slight decoration inside the rim, either incised with a sharp tool or impressed with a toothed ' rocker ' ; the impressions were in many cases filled with a red pigment. The jars Nos. 2 and 3 were black polished and they, too, had a red-filled decoration round the inside of the rim. The necked pot No. 1 was covered with a polished red slip, without mat-marking on the bottom, and is of similar form to the large beer jars found by Garstang in some of the later tombs at Meroë.

As for the ' scraps of silk ' found in the grave, these turned out to be a fluffy mass of some fibrous substance which was not positively identified. Microscopic examination showed it to be neither silk, cotton nor wool, but a straight lustrous fibre unsuitable for spinning. It may have been fibre from the plant locally known as *ushr* and intrusive in the grave. The beads referred to were 3 small flattened ball beads of green beryl, 4 red paste barrel beads and one carnelian bead.

It is difficult to say how much later this grave is than the cemetery on the east bank of the river. I originally dated it, tentatively, to the 4th–5th centuries A.D., but I am now inclined to think this may be too early. The pottery is of a type not found at all in the northern Sudan, nor did it occur at Jebel Moya, but it has affinities with some of that found on the habitation site at Abu Geili. There the impressed decoration on the pottery was usually filled with red pigment, and a device such as that on the pot No. 3 in FIG. 6 was a basic element in the decorative designs. Black-polished carinated bowls were also common at Abu Geili, and the form No. 5 in FIG. 6 suggests this shape, though it is more likely to be a recession from, rather than an advance towards, the sharply defined carination. On the other hand, bulbous, narrow-necked pots such as No. 1 in FIG. 6 did not occur at Abu Geili and, in general, the pottery on that site was of better quality than that from the grave under discussion. There were also found at Abu Geili fragments of ' Jebel Moya ' ware and of recognizably Meroitic pottery—wares which did not appear to be in use at the time the grave on the west bank was dug. The evidence, such as it is, suggests that this grave belongs to a period contemporary with the later occupation of the Abu Geili site and may therefore be dated to the 6th–7th centuries. There is, it must be admitted, as yet no means of dating finds of this kind with any approach to accuracy.

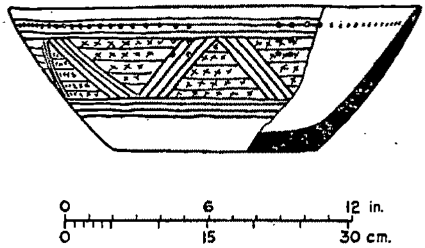

FIG. 7. BOWL FROM FUNG GRAVE, SITE 4, SENNAR

We turn now to the three burials noted by Mr Arkell and published by him in *Sudan Notes and Records* (12). The first of these was found on the west bank of the Blue Nile on the site of the old Fung city (Site 4 on the map, FIG 2). It had been exposed by a fall of the steep-sided river bank and was found in the cliff face about three feet from the top. The body appeared to have been buried in a fully extended position, and inverted over the head and shoulders was a broken, flat-bottomed bowl, FIG. 7. (This figure is based on the sketch and dimensions published by Mr Arkell). This

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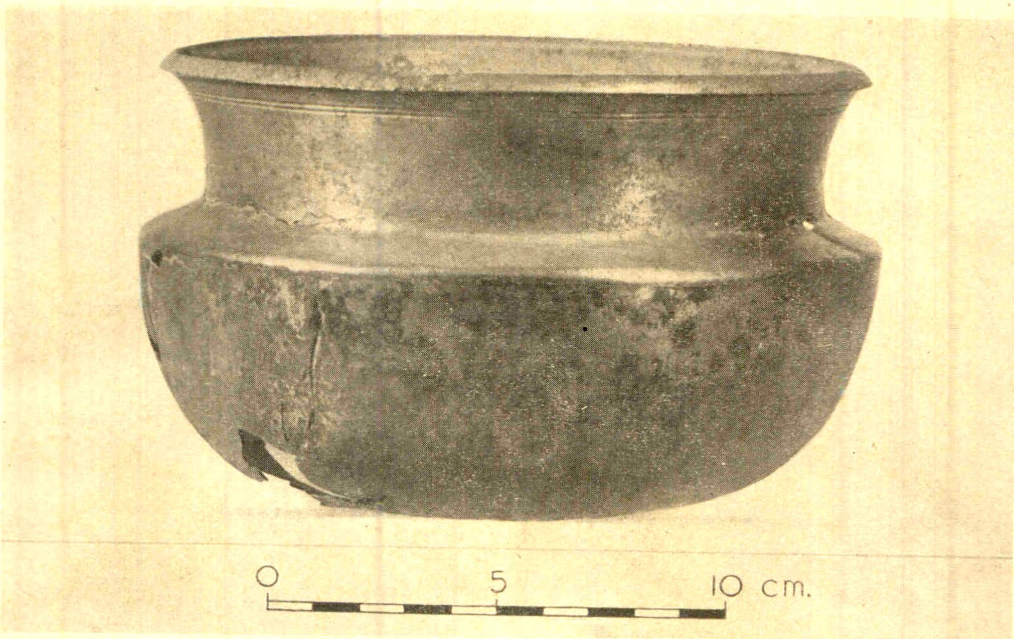
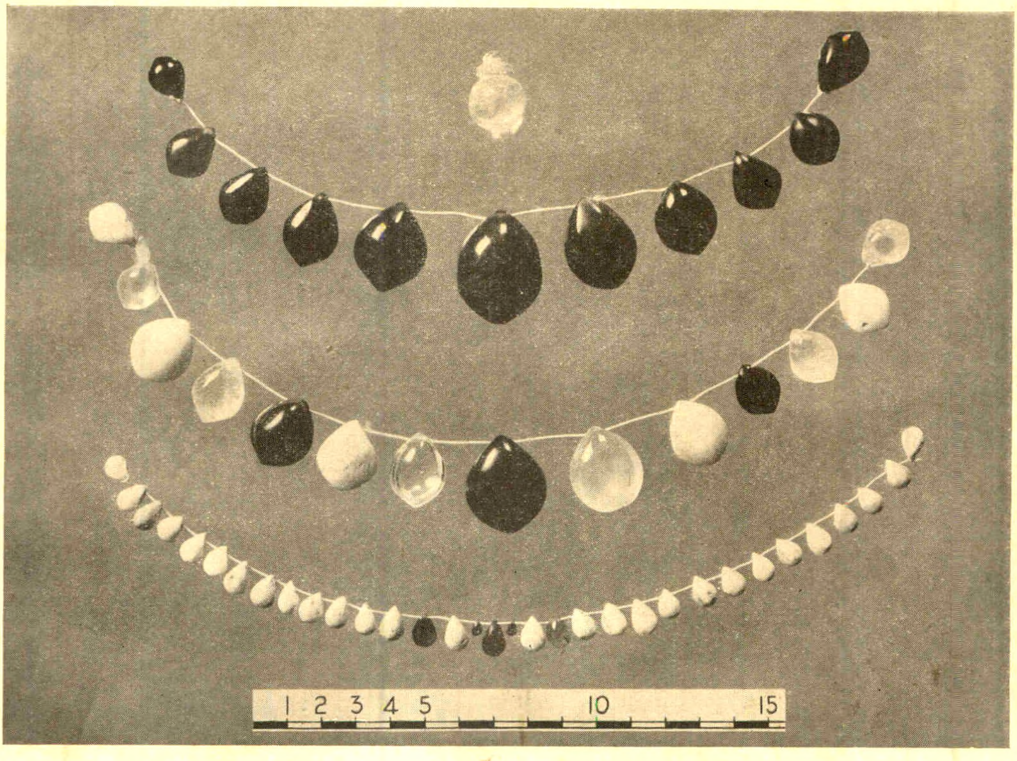
[12] Vol. XVII, 1934, p. 103.



a. BRONZE BOWL FROM THE MEROITIC CEMETERY AT SENNAR, EAST BANK



b. PENDANTS FROM THE SAME CEMETERY (scale in centimetres)

facing p. 20

bowl was hand-made, of coarse, black pottery, ill-fired and unpolished, and on the outside was a deeply but roughly incised geometrical design. There was a row of holes below the rim for most of its circumference, and there were two sets of six holes for some purpose unknown. Inside (? underneath) the bowl were found 28 small glass beads, brown, green, blue and black, and an ostrich egg shell disc head.

This burial is exactly like many of those in the Abu Geili cemetery (**13**). On that site flat-bottomed bowls of the same general type as that shown in FIG. 7 were found in most of the graves, and in several cases they were inverted over the heads of the bodies in just the same manner as that described by Mr Arkell. The burial therefore unquestionably belongs to the same period as those in the Abu Geili cemetery, namely, to the Fung period, 16th–18th centuries.

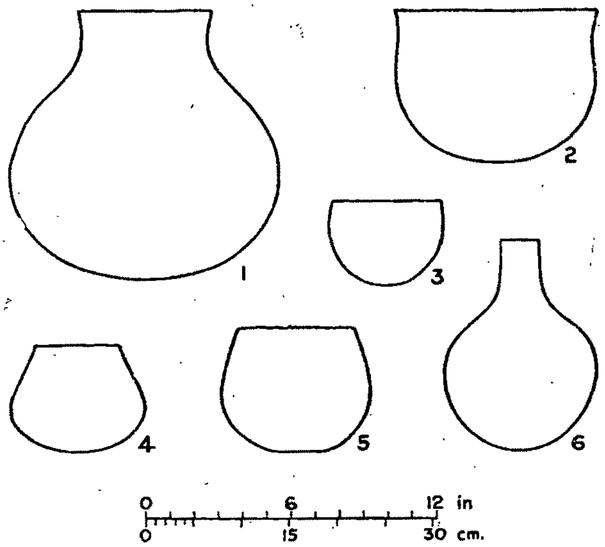


FIG. 8. POTTERY FROM GRAVE, SITE 5

The second burial mentioned by Mr Arkell was exposed by the fall of the Blue Nile after the 1931–2 flood, on the west bank of the river about a quarter of a mile north of the Sennar dam (Site 5). On clearing the grave the skeleton was seen to be lying on the right side, with the knees bent but not in a fully contracted position, head to the north and facing west. The hands were raised in front of the face and were holding the pot No. 4 in FIG. 8. The other pots shown in this figure were also found in the grave; all of them were hand-made. (The outlines in FIGS. 8 and 9 have been drawn on the basis of the measurements and photographs published in *Sudan Notes and Records*). The jar No. 1 was of red ware, and all the rest of black ware, unpolished except for Nos. 4 and 5. The latter had a notched rim. No. 1 had a double band of incised decoration round the rim on the outside, and No. 2 had a band of incised cross-hatching under the rim on the outside and a slight stabbed decoration inside the rim. The remaining pots were undecorated. Also in the grave were five barrel beads of white quartzite and a few

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[13] *Abu Geili*, by O. G. S. Crawford and F. Addison (*The Wellcome Excavations in the Sudan*), pls. XIII, XIV.

p15609

coloured glass beads which, from their position, had probably once formed a bracelet round the right wrist of the body.

This grave was found not far from that discovered in 1925. Whether or not the two belonged to the same cemetery is not certain, as the position of neither was accurately plotted, but it is evident that they belonged to much the same period. Mr Arkell observes that near the grave on the foreshore were found a number of potsherds of finer ware, suggesting that an earlier, Meroitic, cemetery had been eroded away by the river.

The third grave which Mr Arkell noted was found (? in 1932) a yard below high reservoir level on the west bank of the river about half a mile north of Wad Hashim village and south of the dam (Site 6). It was disclosed as a result of erosion caused by the filling of the reservoir. The grave was oval, the body having apparently been buried in a sitting position, and adjoining it to the north was a circular hole in which were found the pots shown in FIG. 9. With the body were found ten ground cowries, some tubular beads of green and black glass, some ostrich egg shell disc beads, some 'white' (? quartzite) beads and two small beads of carnelian. Beads of much the same kind found on the surface in the same locality the previous summer indicated that the area was an old burial ground.

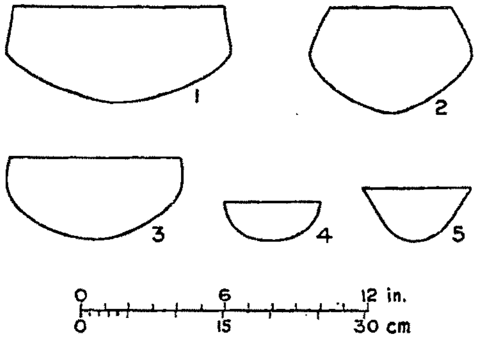

FIG. 9. POTTERY FROM GRAVE, SITE 6

The pottery was all black-polished, with a more lustrous finish than that found on any pottery from the sites, including Abu Geili, hitherto mentioned. The bowl No. 1 in FIG. 9 had four small knobs equally spaced outside on its greatest circumference and had an incised decoration inside and outside. The bowls Nos. 2 and 4 had one knob and No. 2 had a band of simple incised decoration below the rim. This pottery is different from any so far mentioned and resembles some found by Garstang in a late cemetery at Meroë itself, and some accidentally discovered at Shendi. It is post-Meroitic in date and, though it cannot definitely be called Christian pottery, it probably belongs to the period when the Christian kingdom of Aloa, with its capital at Soba, had already become established. It may be regarded, without very strong evidence, as later than that from Sites Nos. 3 and 5, possibly 7th–8th centuries.

It is useless to pretend that these various discoveries shed more than a dim and fitful light upon the dark places of the history of the Sudan. The available material does not enable us to form a complete, or even recognizable picture, but provides only a few shadowy intimations like those which first appear during the development of a photographic plate.

A glance at FIGS. 3, 6, 7, 8 and 9 shows that we have to deal with four groups of pottery which have little or no connexion with each other, and the differences between these groups are to be explained by a movement of peoples or tribes rather than by any process of evolution. The earliest group is that from the Meroitic cemetery, Site 2, and consists of wheel-made ware imported from the north and local hand-made pottery. The affinities of this latter are with some of that found at Jebel Moya and not with anything from Abu Geili. Some centuries later appears the second group consisting of the pots from Sites 3 and 5; later still comes the pottery from Site 6, and nearly a millennium after this come the Fung wares from Site 4 and the cemetery at Abu Geili.

The relationship between the habitation site at Abu Geili and the town which must have belonged to the Meroitic cemetery (Site 2) is difficult to make out. The characteristic pottery of Abu Geili is of a kind found neither in this Meroitic cemetery, nor in the northern Sudan, nor at Jebel Moya; it was therefore probably introduced to the district by a people advancing from the south or south-east. Also, the type of local pottery found in the Meroitic cemetery was not found to any appreciable extent at Abu Geili. It might seem, then, that invaders from the south first overwhelmed the hypothetical Meroitic town (which, to save circumlocution, I will call Early Sennar) and then founded a settlement of their own close by. The event, however, was probably not quite as simple and clear-cut as that. Amulets (14) and other small objects were found in the lowest levels at Abu Geili of a kind to suggest that a settlement existed there in early Meroitic, if not in Napatan, times, and pendants of the kind illustrated in the PLATE, b were also found in small numbers on the site (15). This settlement, then, was already in existence when Early Sennar was founded, possibly, as suggested earlier, as the seat of a governor of the southern provinces. The new town, it may be assumed, absorbed the few people then living at Abu Geili and this latter site was for a time deserted. The first appearance of the advancing southerners may well have been in the nature of an infiltration rather than an advance in force, and they may have been allowed to settle, without interference, on the deserted site at Abu Geili so that two towns would exist not far apart. It would not, in that case, be surprising if a few beads or an occasional carnelian pendant found their way from Early Sennar to Early Abu Geili. Later arrivals from the south would tend to join their kinsmen, and their settlement at Abu Geili would grow in size and importance. In course of time, perhaps about the end of the second century A.D. when the kingdom had begun to decline towards its fall, the Meroitic outpost at Early Sennar was abandoned and the governor withdrawn. Those of the original native population who were not assimilated probably migrated northwards and the country was given over to those southern tribes which had moved in long before the Axumite invasion of A.D. 350. Abu Geili, however, continued to flourish for some centuries to come, and beads and other objects found there show that it maintained some sort of trade connexion with the north.

The second group of pottery, that from the graves on Sites 3 and 5, is in the same tradition as much of the Abu Geili pottery, though somewhat debased. The people who made it were probably of the same racial stock as those in the Abu Geili district, but there is nothing to show whether they left that region for the west bank of the river at a time fairly late in the history of Abu Geili, or whether they were late comers direct from the south.

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(14) *Abu Geili*, pl. LA.

(15) ibid, pl. LB.

The pottery from Site No. 6 appears to have no connexion with Abu Geili. It does not occur in the northern Sudan but, being found in places as far apart as Meroë and Wad Hashim, it may not have originated actually in the south but may have been developed in the neighbourhood of Meroë or in the Gezira (16). In both those regions there were settled at the time people originally of southern stock who would be familiar with the technique of making black-polished pottery and capable of adapting it to new forms. It is therefore possible that the pottery from Site No. 6 may be an indication of a drift of people backwards towards the south. All this, however, is highly speculative.

Between this pottery and our latest group—the Fung pottery from Site No. 4 and the Abu Geili cemetery—there is a gap in time of several hundred years, and chance has provided no material to illustrate whatever ebb and flow of peoples along the Blue and White Niles there may have been during this period. The Fung pottery represents the last advance of any consequence from the south, and, after the power of the Fung kings had declined, the history of the negroid tribes in the Sudan until nearly the end of the 19th century was one of retreat before the Arab slave traders into the swamps of the White Nile and the fastnesses of the Nuba Hills.

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[16] The area in the angle between the Blue and White Niles.

Tables in Roman Britain*

by JOAN LIVERSIDGE

IT is but rarely that we light upon any trace of those articles of furniture which must have been in daily use in every Romano-British home. The purpose of this article is to call attention to a series of objects which contribute something to our scanty knowledge of this subject, namely the carved fragments of Kimmeridge shale occasionally found during the excavation of Romano-British sites. Of these no less than seven are on view in the Dorchester Museum (1). All were found at or near Dorchester, and there seems no reason to doubt that they were made locally, as a shale industry is known to have existed in Dorset since Neolithic times.

The best known piece is probably the one discovered at Colliton Park, Dorchester, in a rubbish pit 16 feet deep, lying beneath the hypocaust of one of the houses of the Roman town of Durnovaria (PLATE I, 1) (2). It consists of the leg of a piece of furniture with a tenon top made to fit into a mortise in some flat surface. The upper part of the leg is carved to represent an animal's head with an open mouth and a lolling tongue, the latter being extended in an unrealistic manner to join the leg, which swells out to meet it. Below the animal's head the leg is cut away in a semi-circle at the back and shows in front a fluted band which may perhaps be a reminiscence of some form of foliate ornamentation. Behind the animal's head is a small notch. The leg itself terminates in a claw foot. Part of another leg of this type, a little smaller in size, was found on the villa site at Frampton near Dorchester (PLATE I, 2). This is carved with a head resembling that on the Colliton Park leg, but here a space has been hollowed out between the elongated tongue and the animal's neck, and the band of fluted ornamentation is missing. A third example was discovered during the excavation of the villa at Preston, near Weymouth (PLATE I, 3) (3). Of this fragment only the left-hand side is intact, the right-hand side having split away in an almost straight line. The head survives, but the tenon has been broken off, and most of the animal's tongue has disappeared. On the fragment of a fourth leg which comes from South Street, Dorchester, the extended tongue survives, although the carved head itself has unfortunately been destroyed. The lower parts of two legs ending in claw feet were also found in South Street, and another foot of the same type was recovered from the Roman level at Maiden Castle (4).

Objects of Kimmeridge shale are, indeed, by no means confined to the Dorset area, but occur frequently in many parts of the country, usually in the form of armlets and the like. In the Leicester Museum, however, there is the upper half of a carved shale leg with an animal's head very similar in style to that from Colliton Park. It came from the villa at Rothley which was partially excavated in 1901, and may have been among the debris dug out of a well, where the rest of it perhaps still lies concealed. It measures 8½ inches in length (PLATE I, 4) (5). The tenon top is very well preserved; but the main part of the leg has been broken off at the level of the creature's muzzle, at the point where, judging from the other examples, it would have swelled forward into a semi-circle. The carving behind the head may represent a stylized mane.

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* I should like to thank Dr J. M. C. Toynbee for valuable suggestions in connexion with this article.

[1] I am much indebted to Lt.-Col. Drew and to the authorities of the Dorset County Museum, Dorchester, for permission to have these fragments photographed and to publish them.

[2] *Proceedings of the Dorset Natural History and Archaeological Society*, LIX, 1938, p. 9, pl. VIII, c.

[3] *J.R.S.*, XXIII, 1933, p. 206, pl. XXIII.

[4] R. E. M. Wheeler, *Maiden Castle, Dorset* (Report of the Research Committee of the Society of Antiquaries of London, 12), 1943, p. 320, fig. 111, no. 23.

The first question raised by these legs is that of the article of furniture to which they belonged. The excavator of the Preston villa published a conjectural reconstruction (6) in which the fragment from that site features as the front of the arm of a chair ; while the Colliton Park piece has been described as a chair-leg. (See Note 2). But I know of no ancient representations of chairs in domestic use with arms or legs thus adorned ; and a comparison of the Dorchester and Leicester fragments with the remains of actual furniture surviving in other parts of the Roman Empire, or with furniture depicted in works of art, such as sculptured reliefs or paintings, suggests the conclusion that they belonged, not to chairs, but to small tables. Round tables with curved legs ending in claw feet are of frequent occurrence both in Hellenistic and in Roman Imperial times (7). They may have either one or three legs ; and as the Colliton Park and Frampton legs have a notch for a cross-bar behind the animal's head, it seems probable that our examples belonged to the three-legged type. Tables with such legs, but without the animal-head decoration, occur on some Romano-British grave reliefs, such as the two well-known examples in the Yorkshire Museum in York dedicated to Aelia Aeliana and Julia Velva respectively, both showing representations of funeral banquets (8). Animal-heads appear on what may be the representation of a small round table on a fragmentary funeral banquet (?) relief in the Guildhall Museum (9). But for the animal-head decoration in general we must look outside Roman Britain, and the closest parallel to our shale legs so far known is afforded by a recent find made during the excavations of the Casa dell'Atrio Corintio at Herculaneum. This is an almost complete wooden three-legged table, shortly to be published in the *Notizie degli Scavi* by Professor Maiuri, to whom I am greatly indebted for the news of its discovery. Its legs have the usual tenon tops which fit into mortises on the underside of the table. They resemble the Colliton Park and Frampton examples in shape but are naturally of more delicate workmanship, since Kimmeridge shale does not lend itself so well as wood to slender lines, quite apart from the fact that Campanian craftsmen are likely to have been much more skilled than those in provincial Dorset. A detail from a recently published relief

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[5] The other measurements are :—width of the animal's head, across the back of the leg, 3 inches ; side width from the tip of the animal's muzzle to the back of the leg, 3⅞ inches ; width of the leg exclusive of the carving, 2 inches at the top and 1½ inches at the base. After seeing this object in the Leicester Museum and identifying it as a table-leg, I found brief references to it, without illustrations, in W. R. Lethaby, *Londinium : Architecture and the Crafts,* 1923, p. 128 and T. D. Kendrick, *Anglo-Saxon Art to A.D.* 900, 1938, p. 30. Kendrick describes it as made of wood, but further examination has shown that the material is actually Kimmeridge shale. This photograph is published by kind permission of the authorities of the Leicester Museum.

[6] *J.R.S.*, XXIII, 1933, p. 206, fig. 19.

[7] A good account of them can be found in W. Deonna, *Le Mobilier délien* (Exploration archeologique de Dèlos, fasc. XVIII), 1938, p. 43 ff. *Cf.* ibid, pls. XVI, XVII, XIX, XX, XXI.

[8] Gordon Home, *Roman York* 1924, pls. opposite pp. 48, 72.

[9] *Roman London* (Royal Commission on Historical Monuments (England) an Inventory of the Historical Monuments in London, vol. III, 1928, p. 45, pl. IX.

in the Antiquarium Comunale in Rome (PLATE II, B) (**10**) shows carpenters working on a wooden table of this type with curved legs decorated with animal heads and joined by cross-bars.

Our next question concerns the species of animal which the carved heads from Rothley and the Dorset sites are intended to represent. On first inspection the Rothley head, in particular, suggests a lion, and indeed the lion was the animal most frequently selected to embellish tables of this sort. It appears, for instance, on a table carved in relief on the Severan Arch at Leptis Magna (**11**), on one carved on the front of the pulpitum of the theatre at Sabratha (**12**), and on one shown in the interior of the Simpelveld sarcophagus (**13**). Another example is depicted on a mosaic pavement from Antioch-on-the-Orontes (**14**). The legs of a fine marble table now in the Museo Nazionale in Naples (PLATE III) (**15**) show lion heads emerging from collars of acanthus foliage, and it was a dim reminiscence of something of this kind which perhaps inspired the fluted ornament on the leg from Colliton Park (PLATE I, 1). Another marble table in the Casa dei Cervi at Herculaneum closely resembles that at Naples, but here it is a panther's head which rises from the foliage (**16**). In the Museum at Sabratha is a beautiful head of a panther made of white alabaster, which clearly belonged to another such table-leg: its shape is not unlike that of the heads from Rothley (PLATE I, 4) and Frampton (PLATE I, 2). None of these animals shows the elongated tongue of the Dorset examples, for this we must turn to the well-known table support from Pompeii (**17**) with winged griffins, which have heads bearing so striking a resemblance to those of the Dorset beasts that we may wonder whether it was griffins rather than lions which the Romano-British carvers sought to portray.

The shale legs from Colliton Park and Preston exhibit another interesting decorative feature when viewed in profile. On the lower half of the Colliton Park example is carved a long slender leg, rising out of the claw foot, and ending in a hoof just below the band of fluted ornamentation. The significance of this is made clearer by the Preston fragment (PLATE I, 3) which consists of the upper half of a leg of which the lower part was broken off just where it began to swell out into a semi-circle. Above the break the leg is embraced between the forepaws of an animal, which is apparently climbing up it, with its flattened outstretched head carved on the front and its shoulders distorted so as to reach to either side. The ears lie back, but the muzzle has been mostly destroyed. One long slender paw can be clearly seen. This representation of an animal climbing up the leg is, so far, unique in this country, but there are earlier examples of it from elsewhere. In the British Museum is the lower part of a wooden leg found at Panticapaeum, in the Crimea (**18**), and carved in imitation of an antelope's foreleg. The shape of this leg,

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[10] *Capitolium*, 1947, p. 21 ff.

[11] *Africa Italiana* IV, 1931, p. 89, fig. 56.

[12] ibid, III, 1930, p. 37, fig. 33.

[13] E. Espérandieu, *Recueil général des bas-reliefs de la Gaule romaine*, XI, 1938, no. 7795.

[14] D. Levi, *Antioch Mosaic Pavements*, 1947, I, p. 187; II, pl. XLII: ' House of the Boat of Psycheo ', A.D. 235–312.

[15] Photo. Alinari, Napoli, 11179.

[16] A. Maiuri, *Ercolano* (Itinerari dei Musei e Monumenti d'Italia, no 53), 1937, fig. 54; *Ercolano* (Visioni Italiche), 1932, p. 73.

[17] Photo. Alinari, Napoli, 11172.

[18] I have to thank the authorities of the Department of Greek and Roman Antiquities of the British Museum for information about this fragment. Cf. G. M. A. Richter, *Ancient Furniture*, 1926, fig. 215, but this is not a very clear picture.

at the point where it swells out in a semi-circle in front of the hock, recalls that of the Frampton and other examples, but here the protruding portion is adorned with a delicate representation of stylized acanthus leaves. From out of the leaves there emerges an animal, the distorted shoulders and slender forelegs of which embrace the table leg in the same way as does the animal climbing up the lower part of the Preston fragment. Unfortunately the animal's head and the upper part of the table leg are missing here, but comparisons with the remains of a three-legged wooden table found in 1842 in a tomb by the Kerch public garden suggest that the animal was probably a greyhound (19), and this identification was indeed adopted by the excavator of the Preston villa (20). A bronze tripod found at Pompeii also has legs of this type (21).

Of interest in connexion with the Panticapaeum leg are the remains of a wooden table found at Luxor in Egypt, and now in the Musée du Cinquantenaire in Brussels (PLATE IV) (22). Part of one leg, and the whole of a second, 0.79 m. in length, survived, as well as the table top which varied from 0.55–0.58 m. in diameter, and was made in two layers with a slight cup-like depression in the upper one. It is supported by legs decorated with swans' heads and necks, rising from acanthus foliage more realistically rendered than on any of the other examples mentioned above, with the exception of the Campanian tables, and ending in an antelope's hoof. The date of this object cannot yet be determined, but it is interesting to find a representation of a very similar table carved on a grave relief of the Roman period now in the Museum of Antiquities in Istanbul (23).

In the case of our British tables of Kimmeridge shale, it is curious that only one fragment possibly belonging to a circular table-top has, so far, been found. Pieces of shale often described as trays, are known from several sites, including the Preston villa but these are usually rectangular in shape and may have been fixed on to articles of furniture as a decorative veneer. The Roman villa at Brading, however, did produce fragments of a circular piece of shale turned on the lathe, and made with what is described as a slight footstand on its underside (24). This might have formed part of a table-top if so it would have been a very small one, as it cannot have been more than 16 inches in diameter when complete.

There is one more shale fragment from Roman Britain which may have belonged to a table-leg. This is a piece from the Braybrooke Collection, found on the site of the villa at Foscott in Buckinghamshire, and now in the Museum of Archaeology and Ethnology in Cambridge (25). It measures about 3 by 4 inches and has the usual tenon top, and like the Preston fragment one side has split away in an almost straight line. The surviving piece shows in place of a carved animal's head a curious spiral motif. It is possible that this may be a tail, perhaps of some kind of sea-beast, but if so, it would be the tail of an animal climbing down the leg, and not up it, and there seem to

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[19] S. Reinach, *Antiquités du Bosphore Cimmérien*, 1892, pl. LXXXI; Sir E. H. Minns, *Scythians and Greeks*, 1913, p. 333.

[20] *Proceedings of the Dorset Natural History and Historical Society*, LIV, 1932, p. 27.

[21] V. Spinazzola, *Le arti decorative in Pompei e nel Museo Nazionale di Napoli*, 1928, pl. 263.

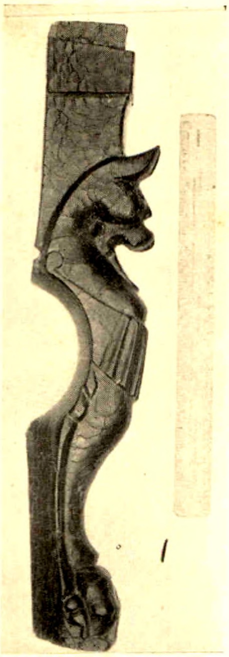
[22] This photograph is published by kind permission of the authorities of the Musée du Cinquantenaire. Cf. Richter, op. cit., fig. 213.

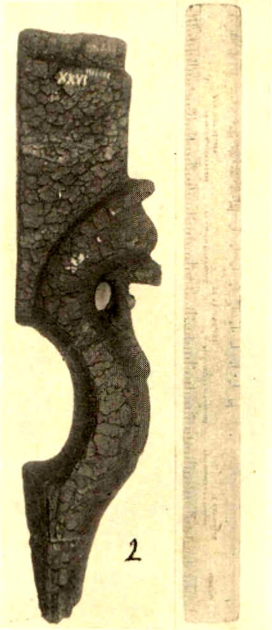
[23] Richter, op. cit., fig. 214.

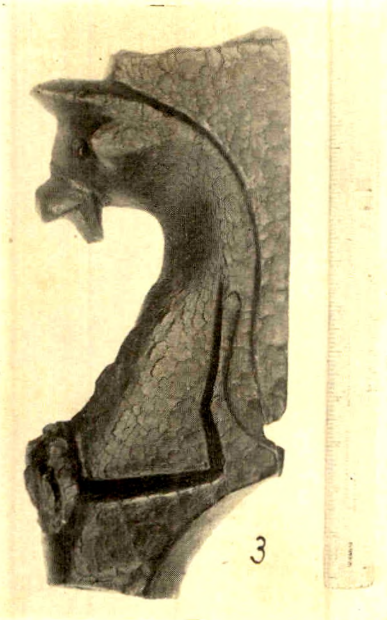
[24] J. E. and F. G. H. Price, The Roman Buildings at Morton, near Brading, 1881, p. 25.

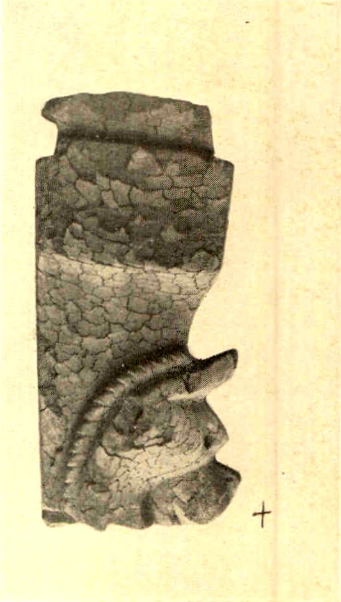
[25] For an account of the villa, see *Gentleman's Magazine* 1838, I, p. 302; 1841, I, p. 81; 1843, I, p. 303.

PLATE I









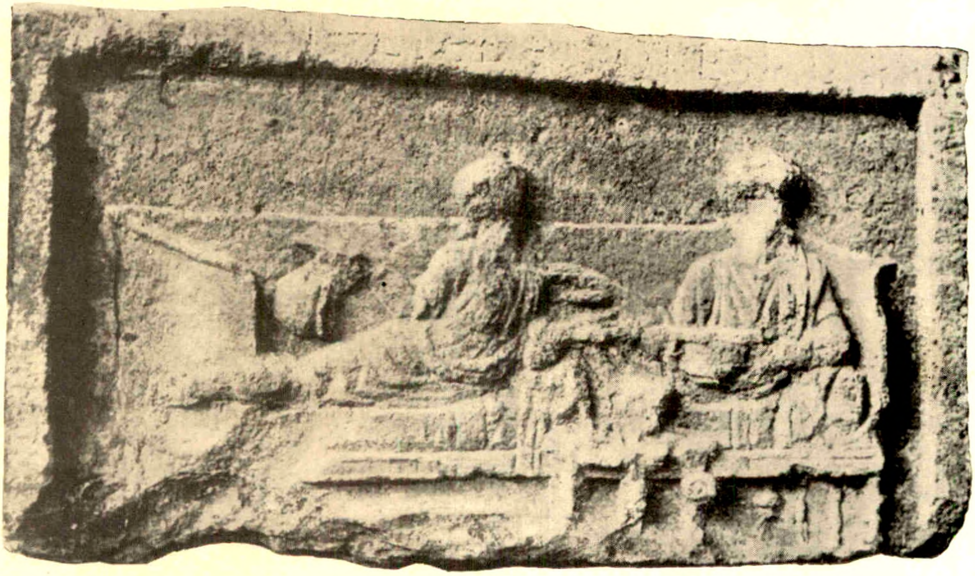
ROMAN TABLE-LEGS

1. Colliton Park, Dorchester
2. Frampton, Dorset
3. Preston, near Weymouth, Dorset
4. Rothley, Leicestershire

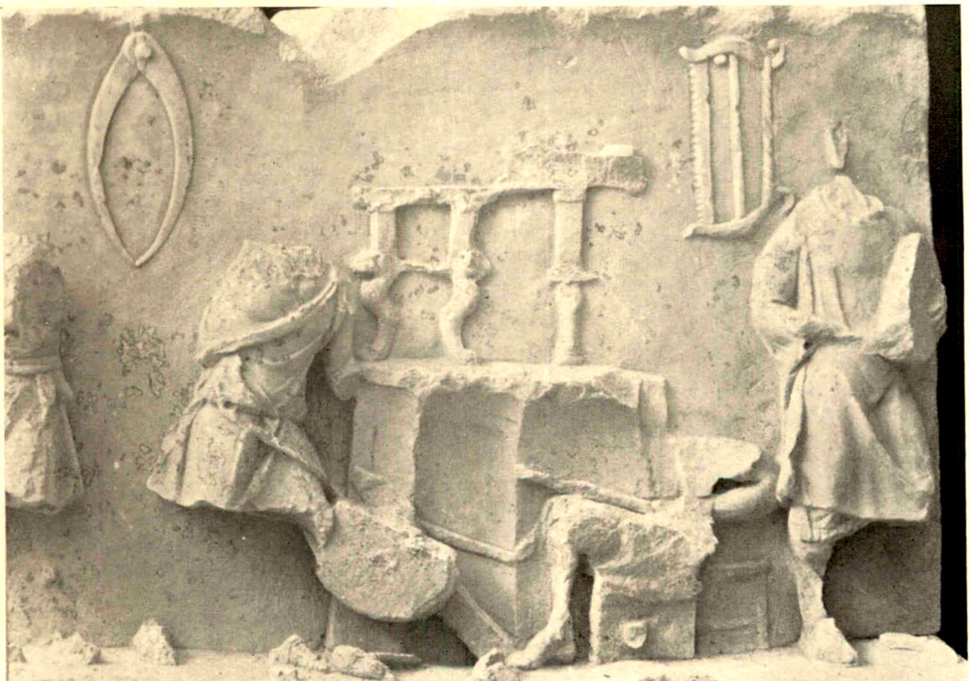
The ruler is 12 inches (30 cm.) long

facing p. 28

PLATE II

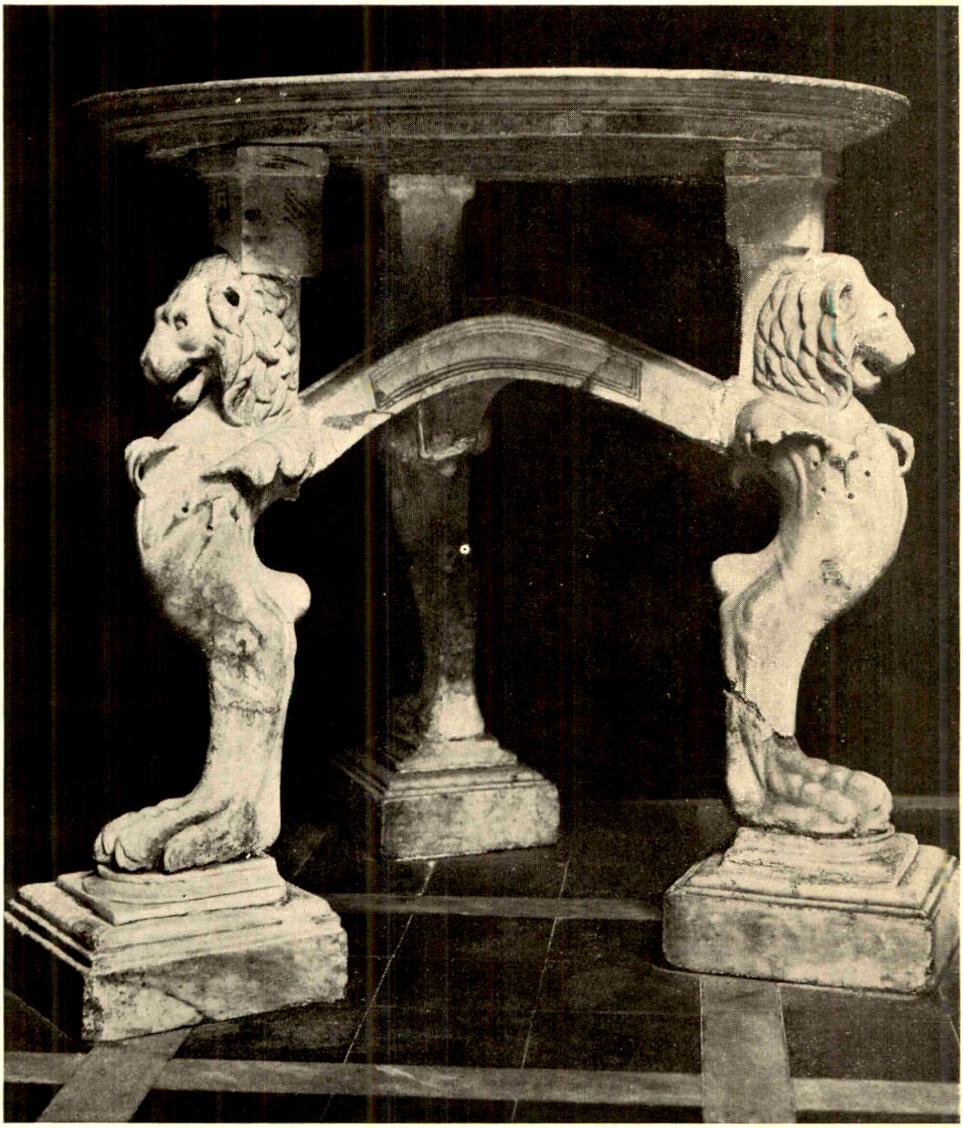


a. GRAVE RELIEF FROM NARBONNE (Musée Lapidaire, Narbonne)



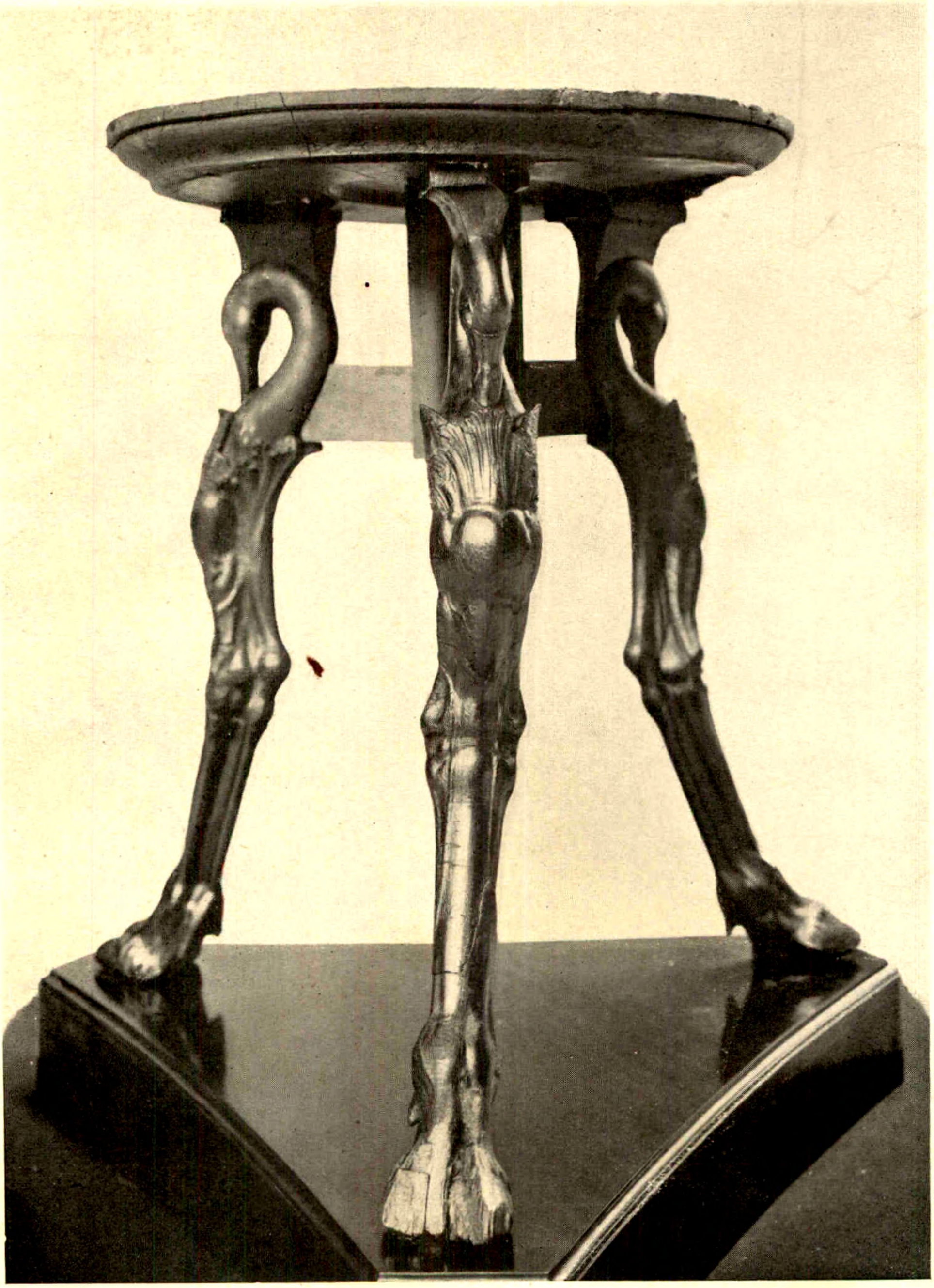
b. RELIEF IN THE ANTIQUARIUM COMUNALE, ROME

PLATE III



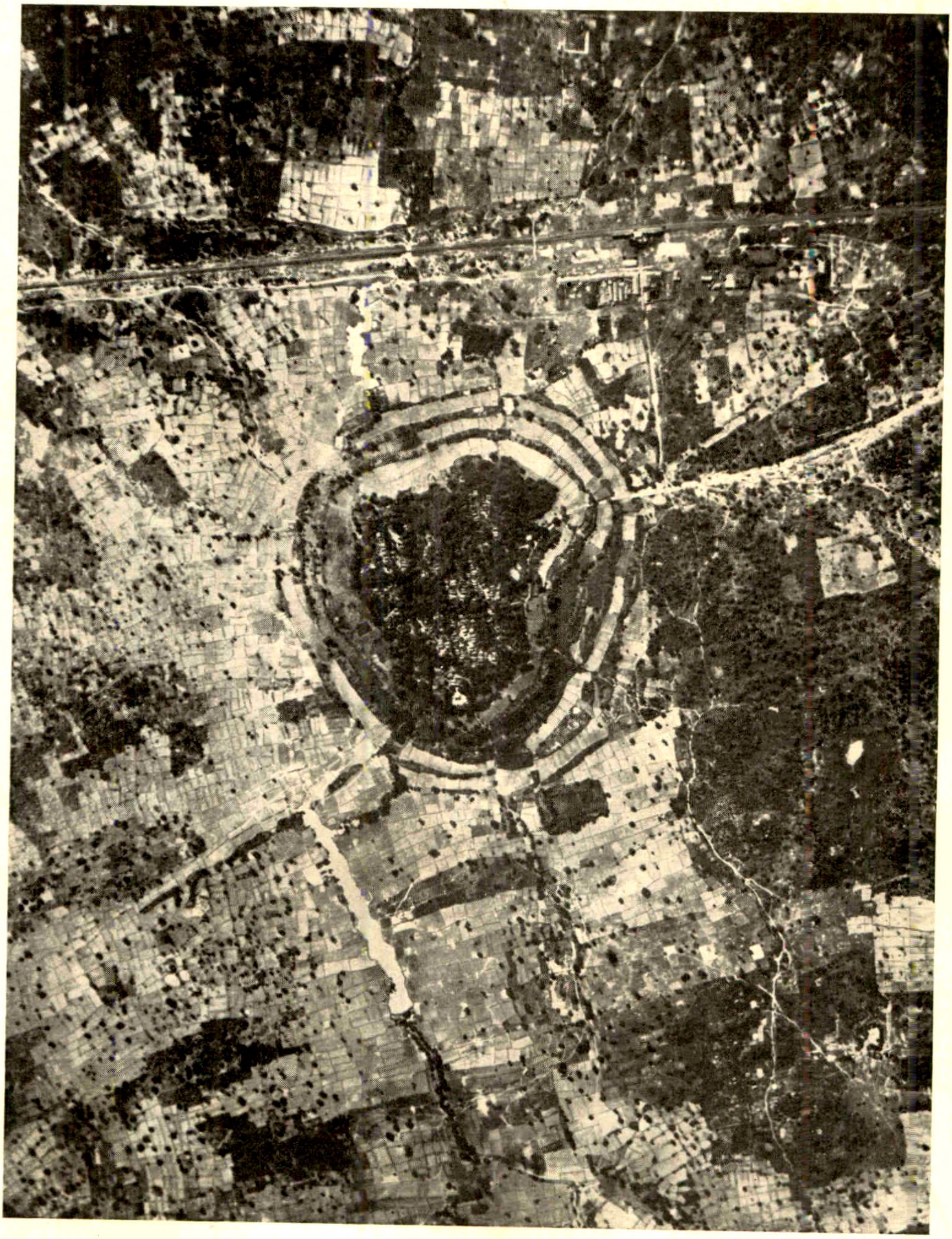
MARBLE TABLE IN NAPLES MUSEUM

PLATE IV



WOODEN TABLE FROM LUXOR (reconstructed)
(Musée du Cinquantenaire, Brussels)

PLATE V



BAN THAMEN CHAI, 15° 01′ N., 102° 55′ E., BURIRAM PROVINCE, EASTERN SIAM
A TYPICAL MULTIPLE RAMPART SITE
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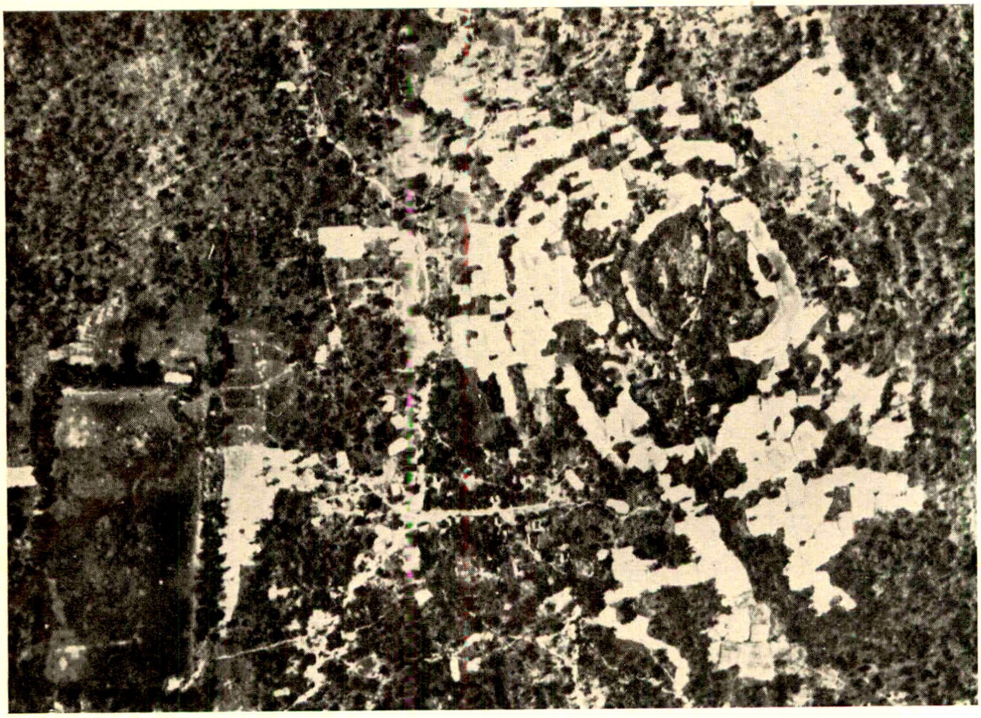
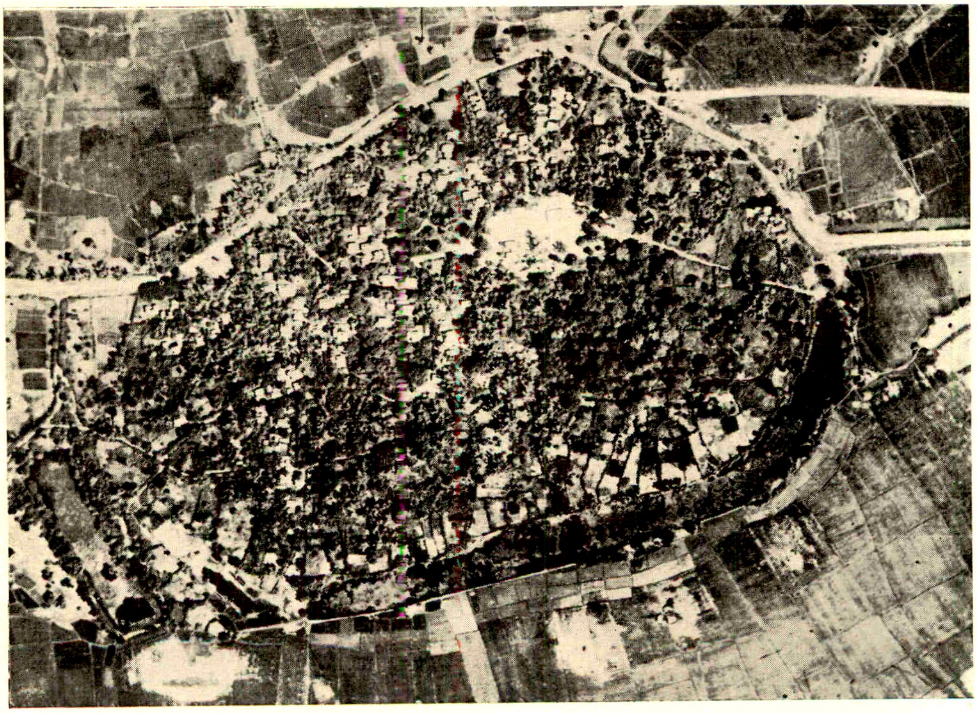
PLATE VI



a. AN UN-NAMED SITE PARTIALLY DESTROYED BY AGRICULTURE IN BURIRAM PROVINCE, EASTERN SIAM



b. A DEFENDED ROCK OUTCROP SITE NEAR PHRA LAP, 16° 29′ N. 102° 40′ E., IN KHAUNKAEN PROVINCE, EASTERN SIAM

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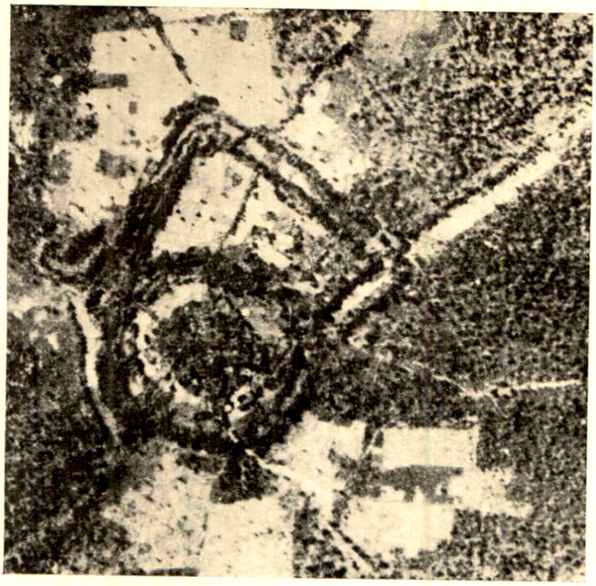
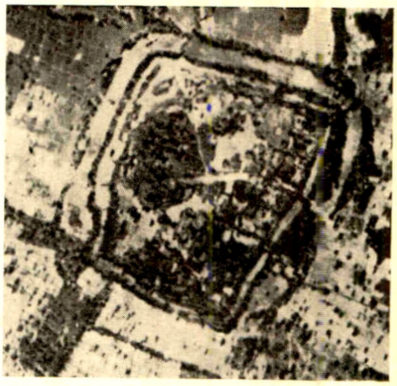
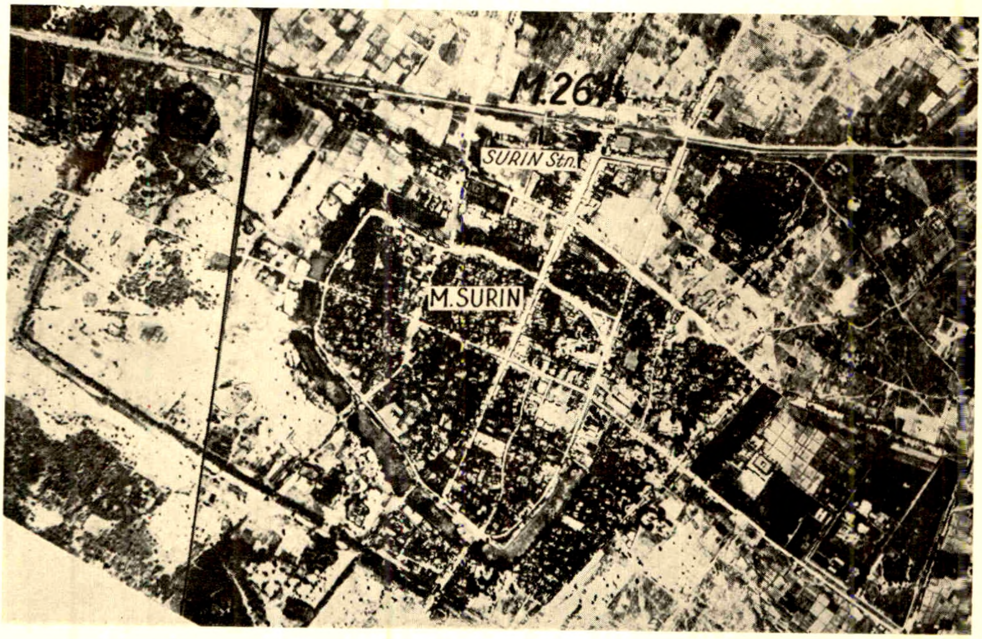
PLATE VII



a. BAN SAI, 14° 41′ N., 103° 04′ E., BURIRAM PROVINCE, EASTERN SIAM. A CIRCULAR EARTHWORK WITH LATER RECTANGULAR ADDITION



b. NEAR BAN SAUK, 14° 59′ N., 103° 35′ E., SURIN PROVINCE, EASTERN SIAM. A SMALL IRREGULAR EARTHWORK ENCLOSED IN AN EARLY ATTEMPT AT REGULAR PLANNING



c. MUANG SURIN, 14° 50′ N., 103° 30′ E., A ROCK OUTCROP SITE WITH LATER MORE REGULAR ADDITIONS

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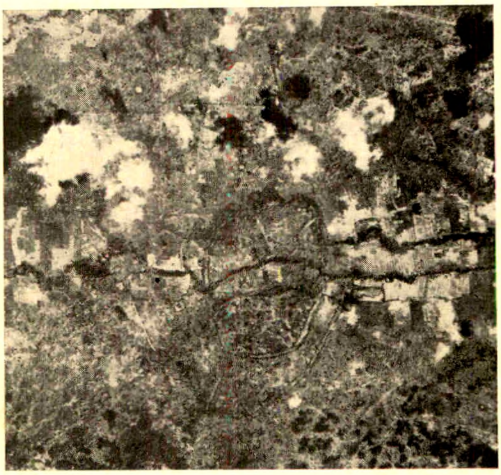
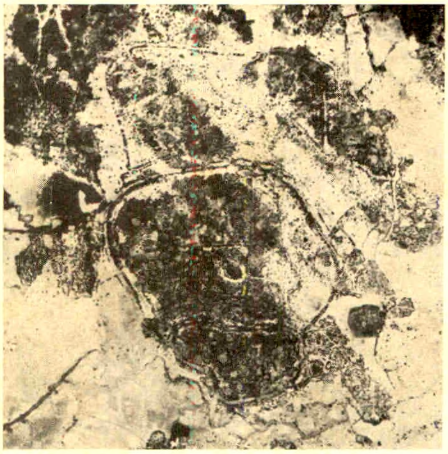
PLATE VIII



a. A LARGE TOWN SITE SHOWING SIGNS OF PROLONGED OCCUPATION FOUR AND A HALF MILES WEST OF ARANYA PRATHET, PRACHINBURI PROVINCE, EASTERN SIAM



b. MUANG SIMA, 14° 55′ N., 101° 46′ E., NAKHAUN RACHASIMA PROVINCE, EASTERN SIAM, AN EXTENSIVE, ABANDONED CITY SITE COMMANDING ONE OF THE MAIN PASSES INTO EASTERN SIAM

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be no other examples of such a variation. As a purely ornamental motif the spiral is found on bronze legs in the Naples Museum (26). But the closest parallel is provided by a grave relief from Narbonne which shows a funeral banquet with a three-legged table of the familiar type (PLATE II, A) (27). The carved decoration has unfortunately only survived on one leg, but there it seems to consist of a spiral similar to that on the Foscott fragment.

We are able, then, to form a fairly clear idea of at least one item of domestic furniture current in Romano-British town and country homes, namely the small three-legged table of shale. The material and workmanship of our surviving fragments alike proclaim their local origin. But in shape and in the character of their ornamentation they mostly conform to a well-established, classical type widespread throughout the Roman world.

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[26] H. Roux, *Herculaneum et Pompéi*, VII : *les bronzes*, 1840, pl. XCVIII.

[27] Espérandieu, op. cit., I, 1907, no. 643.

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Irregular Earthworks in Eastern Siam: an air survey

by P. D. R. WILLIAMS-HUNT

DURING the war and in the immediate post-war period the Royal Air Force and the United States Army Air Force undertook a considerable air survey programme covering Burma, Siam and French Indo-China. In Siam the initial cover was restricted to large-scale (1:15,000 and better) photographs of towns, airfields and communications. Later a more ambitious programme of survey cover (scales 1 : 50,000 and 1 : 25,000 with a few towns and beaches at larger scale) was undertaken, and practically the whole of the country has been covered by air photographs of one scale or another.

It has been my privilege to serve with the R.A.F. in Siam on both ground and flying duties and, more recently, to be in a position to examine most of the photographs taken. A very considerable number of archaeological sites have come to light, many being noted for the first time; and it is my intention in this initial paper to comment briefly on one particular type of earthwork which appears to have a limited distribution in eastern Siam (1). The air photographs are reproduced with the sanction of the Air Ministry.

It must be emphasized that although Siam, the meeting place of Indian and Chinese cultures, is rich in archaeological sites very little systematic work has yet been undertaken. On the one side the natural reluctance of the Siamese to disturb ancient sites and, on the other, comparative absence of trained archaeological research workers have been contributory factors (2). Detailed ground information generally is lacking and it follows that these notes are based on air photographic evidence, in most cases without ground checking, an impropriety of which the writer is only too well aware.

Geographically Siam falls into a number of distinct natural divisions only one of which, the *Eastern Plateau*, concerns us here. This region extending between Longitude 101° and 106° East and between Latitude 14° and 19° North may be described as the *Korat plateau* and the *Korat marginal highlands*, Korat or Nakhon Rachasima (102° 05′ E; 15° 00′ N) being the principal town in this region. The marginal highlands lie on the West, and on the South of the plateau, rise in places to over 4,000 feet and form an exceedingly rugged and forested barrier. The plateau itself is of sandstone composition with alluvial pockets, and its surface vegetation in undeveloped districts is mainly dry monsoon forest although there are some swamp and lake areas. Drainage is to the East and South East into the River Mekong which forms the Eastern and Northern boundaries delimiting Siam from French Indo-China. This river is relatively unnavigable, or was

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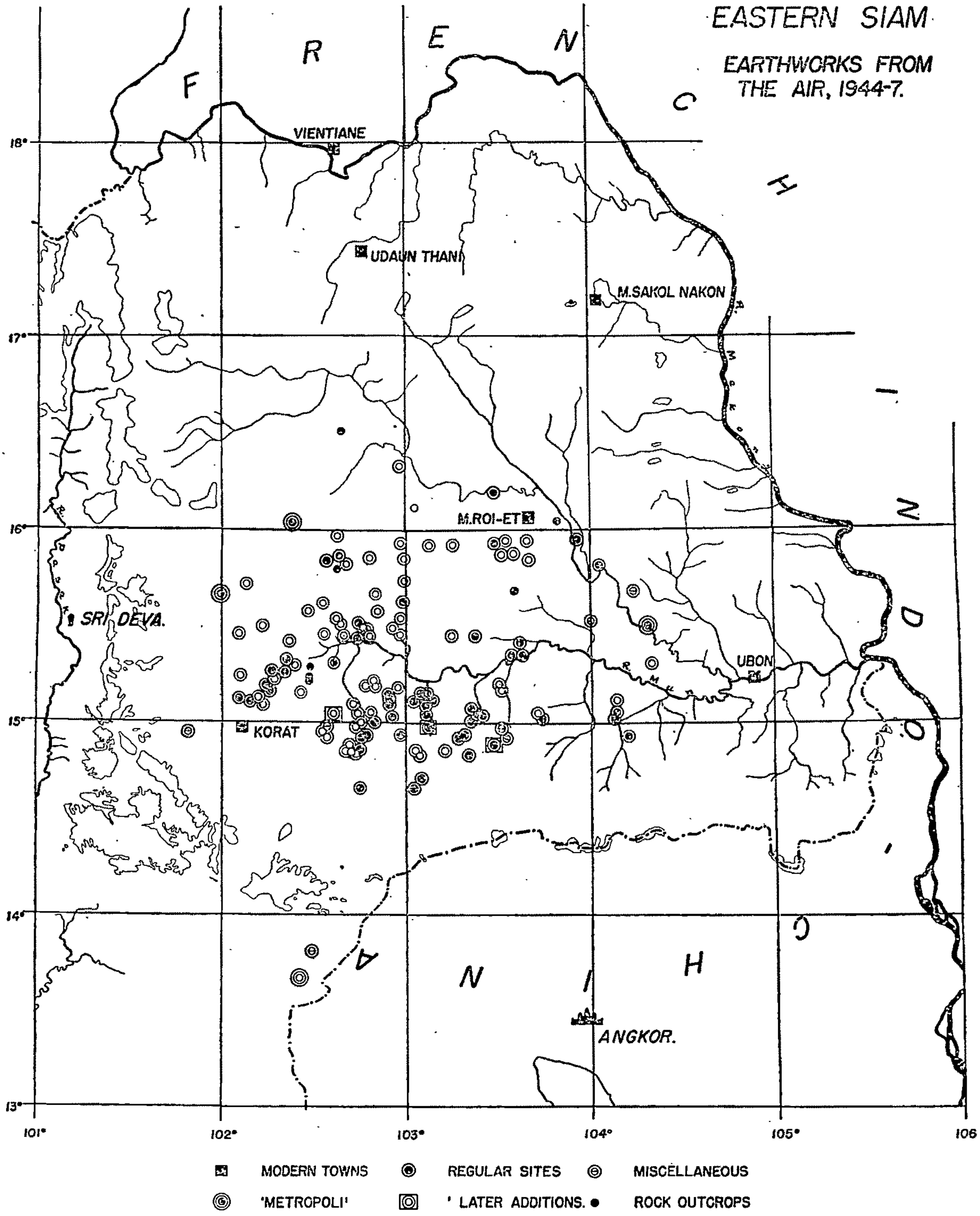
[1] There is a great future for archaeological air work in Siam particularly in view of the difficult ground communications. To arouse interest the writer has prepared an article on the basic theory of archaeological air-photography which is being published in the journal of the Siam Society.

[2] Of course there have been notable exceptions. The names of Coedes, Seidenfaden, le May and the late Prince Damrong may be cited; but most of these men were primarily engaged in other walks of life, and they lacked the time and facilities to undertake the basic fieldwork so badly needed in Siam.



ABANDONED SITES IN OUTLINE. OTHER SITES ARE OCCUPIED BY MODERN SETTLEMENTS

Areas 1500 feet and over above sea level enclosed by contour-lines so at least in ancient times, and is of considerable width. The whole area forms a complete and isolated geographical unit in which a specialized culture might be expected to develop.

Modern communications are few, being limited to two railway lines (Korat-Ubon and Korat-Udorn) and some indifferent roads, although new development is projected. Large scale photographic cover has been restricted to these main communication lines ; but except for a large gap east of Roi-et, an area which appears from ground reports to be comparatively devoid of archaeological sites, and a few other minor gaps, there is complete air cover at 1 : 50,000 scale. This is a large enough scale to locate the major archaeological sites by size, but isolated temples and shrines are liable to be overlooked. The disadvantages of this type of cover in relation to the type of site described are mentioned in some detail below.

Working away from Siam, on maps, published works and air photographs the archaeologist is beset with numerous problems which do not readily solve themselves. Two of the major problems are bad mapping and wide variation in the spelling of place names. The greater part of the area under review is very indifferently mapped in both British and Siamese versions and there is a distressing tendency to show projected roads and railways as *faits accomplis*. As an example of spelling Korat, on one map it appears as Nakhaun Rāchasīma. Siamese (Thai) being a tonal language with a large alphabet cannot be exactly rendered in English, and the place-names given here must be regarded as an approximation only. (It was originally intended to print the site names in Thai script but typographical difficulties have prevented this).

The net result is that the writer is unable to guarantee his plotting exactly, although it is unlikely that any sites are shown more than four or five miles from their actual location. However, those sites which are described in some detail in the text and illustrated in the plates are known to be accurately pin-pointed. The distribution-map was prepared by initial plotting on maps of quarter-inch to one mile scale, and the detail was then transferred to a one million scale map. At this scale, even if the plotting is five miles out, the symbols employed are sufficiently large to cover the correct site locations.

The total number of archaeological remains is enormous. In some areas there is scarcely a square mile in which there are not at least three or four sites of varying size and date. Detailed analysis from the air will take many months and there is the work of a lifetime on the ground. The vast majority of sites are of a rectangular outline aligned on a roughly East-West axis. Many of these are comparatively modern but a large number have the characteristic Khmer format typified at Angkor. To the writer, however, the main interest lies in a compact group of irregular earthworks which appear to pre-date the much publicized works of the Khmer empire. These more primitive structures appear to have escaped notice as a group and it is with these that this paper is concerned. From the air they may be classified as follows :—

(*a*) Some two hundred small defended towns characterized by multiple, concentric earthworks and very regular almost circular construction, obviously forming a single complex.

(*b*) Four ' metropolis ' sites of similar type but with a wide gap between the inner and outer ramparts.

(*c*) Several sites located on rock outcrops probably forming part of the same group.

(*d*) A number of irregular earthworks widely varying in layout and apparently not associated with the above three groups. Sri Deva is the main site in this category.

Basic ground surveys in this area have been undertaken by de Lajonquière (3) and Seidenfaden (4) but Quaritch Wales has been the only expert European archaeologist to carry out any recent work (5). Seidenfaden's work is in the form of brief notes supplemented by a very useful map. Certain irregular earthworks are mentioned, some with brick ramparts, but in no case do finds appear to antedate the Khmer period (6). Quaritch Wales is mainly concerned with Sri Deva, a city of the Founan empire, probably founded in the first half of the 5th century. Sri Deva has an evil reputation on the ground. It appears to have an equally unfortunate effect on air-photography, since of the half dozen or so photographs taken not one is sufficiently clear for reproduction. Sri Deva is of importance in that it is the only irregular earthwork in this region for which there is a date. However, it is very considerably larger than those in the groups described below and is unique in its isolation. From the air view it stands in a class on its own. It might be added that the air view discloses some minor variations in layout from the published map.

Ban Thamen Chai, 15° 01′ N., 102° 55′ E., in Buriram Province, may be regarded as typical of the sites in group *a*. (PLATE V). It lies immediately north of the Korat—Ubon railway line, some fifty miles east of the former town, and is occupied by a modern village. Originally there appears to have been a quadruple series of concentric ramparts each about one hundred feet apart but these have been badly cut about by modern paddy fields. The whole site is roughly 800 by 960 yards in extent. It is not now possible to determine from air-photographs the locations and nature of former entrances but there are traces of one at the North—i.e. at the apex of the oval—and another at the South-East corner. The silted stream probably originally flowed through the ditches, portions of which still contain a considerable amount of water.

PLATE VIa shows an un-named site about 14° 55′ N., 103° 17′ E. midway between the towns of Buriram and Muang Surin. Originally there appears to have been a single circular earthwork some 550 feet in diameter, with a ditch, still waterlogged, now just under one hundred feet wide. Later at least two concentric ramparts were constructed traces of which can still just be seen. The large rectangular tank probably dates from late Khmer days. The whole site has been broken up by paddy fields. This is an example of difficult air appreciation; visually or on smaller scale photographs it might well be missed and some of the gaps in the distribution map may be explained in this way.

Many sites show modification of plan, either by additions of earthworks on one side or by total enclosure within a larger rectangular structure. Unfortunately most of these sites were recorded from the air when conditions of ground haze prevailed and the resultant air-photographs cannot be produced satisfactorily. The two examples shown in PLATE VII can not, therefore, be regarded as entirely typical.

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[3] L. de Lajonquière, *Inventaire Descriptif des Monuments du Cambodge* (3 volumes, 1902–11) and *Carte Archeologique du Cambodge*, 1910.

[4] Erik Seidenfaden, *Complement a l'Inventaire Descriptif des Monuments du Cambodge*, Bulletin de l'École Française d'Extreme-Orient, Tome XXII, 1922.

[5] H. G. Quaritch Wales, *The Exploration of Sri Deva, An Ancient Indian City in Indochina*, Indian Art and Letters, vol. X, no. 2, 1936.

[6] The writer has been in correspondence with Major Seidenfaden, now living in retirement in Denmark; but although many useful suggestions have been offered no additional evidence as to the date of these sites is forthcoming. Seidenfaden suggests a parallel to some of these sites amongst the Lawa villages in the Mehongson district of northwest Siam, but these appear somewhat different from the sir. R. le May, an authority on Siamese art, has also examined some of the air-photographs but was unable to comment on the sites.

Ban Sai, 14° 41′ N., 103° 04′ E., in Buriram Province, appears to have been a regular earthwork with a double rampart to which a more regular structure has been added (PLATE VIIa). The whole site is reminiscent of Sri Deva with its main town and traders' annexe, although Ban Sai is only some 1,100 yards across and is, of course, on a very much smaller scale. It is not possible to say from air-photographs whether the small double feature to the left of the site is the remains of a former outer earthwork surrounding the circular portion of the town or part of a defended entrance.

There are several double sites of this type; the additions ranging from irregular ovals to the more regular pattern typified at Ban Sai.

The second site is un-named on the map and lies about a couple of miles to the South of Ban Sauk, 14° 59′ N., 103° 35′ E., in Surin Province (PLATE VIIb). Originally there appears to have been a small multiple earthwork—centre left—which has been subsequently enclosed within a triple and roughly rectangular series of ramparts. This site is more or less on the general alignment of Khmer sites and may well represent a transitional stage of early Khmer times. Raised ramps of some type emanate from the main earthwork and require investigation on the ground.

There are a number of sites of this nature in which the original series of circular earthworks or the rock outcrops described below have been incorporated into larger Khmer works. Muang Surin (PLATE VIIc), 14° 50′ N., 103° 30′ E., is another typical example. Here the shape of the original nucleus has probably been dictated by a rock outcrop.

Scattered amongst these more regular multiple earthworks are a number of fortified natural rock outcrops. These sites are exceedingly difficult to see on small scale photographs since every village in this region is under a grove of trees and unless there is a marked height to the outcrop it may well be missed. There are undoubtedly more sites than have been shown on the distribution map particularly in the Northern area.

A typical example is an un-named village immediately west of Phra Lap (Khaunkaen), 16° 29′ N., 102° 40′ E., in Khaunkaen Province (PLATE VIb). The mound is about 700 yards across and is surrounded by a moat averaging 140 feet in width with indications of a second moat at the West end. Two irregular oval enclosures have been added on the North-East side (one is mostly off the photograph illustrated). Many of the sites in this group appear to be rock outcrops forming small islands in swamp or lake areas. The town of Muang Roi-et (not illustrated) is a typical example in which an island in a lake forms the central core around which a town has expanded within a square earthwork of later date. Rock outcrops of this type would naturally be occupied at an early date and might well form the prototype for the earthworks further south.

Around the edge of the main distribution are four sites closely similar in appearance, each consisting of a circular inner rampart and moat and an outer rampart and moat set some considerable distance out. There are indications that there were originally several more of these sites but traces have been removed by agricultural activities. Two of the four share the unusual feature of straddling small rivers—an original characteristic since the ramparts have not been disturbed in any way. Unfortunately these sites are only covered by small scale photographs and the example illustrated (the only site capable of reproduction (PLATE VIIIa) lies away from the main distribution and in some ways is the least typical of the four. It is about 13° 41′ N., 102° 27′ E., some four and a half miles west of Aranya Prathet in Prachinburi Province and might be of later date than the other three. The inner earthwork is a rough oval about 1,400 by 1,200 yards with a moat varying in width from 80 to 160 feet. The outer rampart is rather irregular in its distance from the inner to which it is joined by a straight ditch 80 feet wide. There are now no remains within the inner earthwork which can be located from the air but there are various features of interest outside. These are:—

On the East side, outside the outer moat, two rectangular earthworks, 560 by 400 feet and 900 by 300 feet, together with several smaller rectangular structures probably tanks for water storage, or possibly temples.

On the South side between inner and outer moats a probable temple site with traces of a tank. The main feature is about 250 by 150 feet in extent.

On the West a possible Stupa in an enclosure 250 feet square.

On the North a trapeze-shaped earthwork, 1,000 by 700 yards, with several smaller rectangular earthworks to the south of it.

All these features are probably Khmer or later in date suggesting a prolonged habitation of the site.

Apart from Sri Deva and one other site, the remaining group of irregular earthworks seen from the air in Eastern Siam is of an uninteresting nondescript character and can only be mentioned here. The exception is Muang Sima (PLATE VIIIb) (7), 14° 55′ N., 101° 46′ E., in Nakhaun Rachasima Province, a large irregular earthwork commanding the pass in the marginal highlands through which the Korat-Bangkok railway now runs. Quaritch Wales suggests that the abandonment of Sri Deva might possibly have been due to the opening up of a new trade route through this pass, in which case Muang Sima might be the successor of the former city and thus date from the latter part of the 6th century A.D. The site is well-known on the ground and has been mentioned by both Seidenfaden and de Lajonquière. From the air it presents a complex series of additions to the small irregular earthwork on the South (PLATE VIII), which was apparently the original nucleus. The general impression given is not unlike that of Old Prome in Burma whose final date of occupation does not appear to be much later than the 10th century A.D. Muang Sima was eventually superseded by the present city of Korat.

The difficulties of interpretation mentioned above impose certain limitations in the preparation of an adequate distribution map, and that here given must be regarded as an impression of distribution rather than as the outcome of a final detailed survey. For instance, there are undoubtedly more sites in the area south of Roi-et, but small scale cover of poor quality precludes a definite statement to this effect. The survey cover flown is being made available to the Royal Siamese Survey Department and it is to be hoped that these and other archaeological sites will appear on Siamese maps of the future. Indeed Eastern Siam deserves a special archaeological map series of its own.

It would be tempting but imprudent to speculate on the age of these sites from air photographic evidence alone. There are many unsolved problems in this region, ranging from identification of the bounds of the lost kingdom of Founan to the nature of the early Khmer cities. One thing is perfectly clear, and that is that the distribution of sites here shown corresponds exactly to the present concentrations of population and lines of communication. Then again, the distribution ties up with Quaritch Wales' 'Imperial Trade Route', although it is not clear from his writings how he arrived at his conclusions, which seem to be pure topographical speculation. Further comment would be futile. The excavator's spade alone will provide the final answer.

NOTE.—The Editor of ANTIQUITY has asked me to add a few lines to Major Williams-Hunt's article; and as I served as an Assistant Inspector-General of the Siameses Gendarmerie (from 1907 to 1919) just in those parts of Siam I gladly comply with his

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[7] This site also goes by the name of Muang Nakhon Raxasima Kao.

request. As the space allotted me is very limited I shall do so briefly. To the names of the research workers should be added that of Major E. Aymonier who undertook the first archaeological survey of N.E. Siam in 1884 (see *Le Cambodge*, vol. III); both he and Major Lunet de Lajonquière are considered as prominent archaeologists. The Mekong is navigable all the year round, from Wiengchan in the north to Khone in the south, and large twin-screw steamboats used to ply on it. At present most of the traffic is done by motor along an excellent road skirting the left bank of the great river. With regard to Korat and Nakon Rajasima—the first name is the popular, the latter, the official one. Some of the oval and irregular shaped earthworks mentioned were photographed by the Royal Thai Air Force back in 1922. Most of the one hundred ancient sites visited by me were square in outline. It is my opinion, as far as ethnological and archaeological information goes, that the square earthworks are Khmer, that some of the oval ones are also Khmer, before the Khmer were hinduized. The pre-Khmer earthworks would be due to the Kuis, a Mon-Khmer people preceding the Khmers, while those of the most primitive form *might* be ascribed to a now vanished Melanesian people. The name of Ban Sri Thamenchai is said to be a corruption of Sri Thanon Chai, a famed buffoon at the Royal court in Ayuthia in olden days. The earthworks shown on PLATE VIa are Ban Khu Song Chan (double-moated village) while those on PLATE VIIIa are called Müang Phai.

ERIK SEIDENFADEN.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

THE PLOUGH IN ANCIENT BRITAIN, by F. G. PAYNE. *Arch. Journ.* CIV, 1948, 82–111. [A comprehensive and fully illustrated account by an expert].

THE STONEHENGE CURSUS AND ITS AFFINITIES, by J. F. S. STONE. *Ib. id.* 7–19. [Valuable fresh evidence, particularly about the Blue Stones].

THE HUNTING BATHS AT LEPCIS MAGNA, by J. B. WARD-PERKINS AND OTHERS. *Archaeologia* XCIII, 1949, 165–95; sold separately (15*s*.) by the Soc. of Antiquaries, Burlington House, Piccadilly, W.1, and by Bernard Quaritch, 11 Grafton St., W.1. [A fine example of salvage work, especially the plans; the Italians left no detailed record of their excavations of the Baths].

THE BRITISH SECTION OF THE RAVENNA COSMOGRAPHY, by I. A. RICHMOND and O. G. S. CRAWFORD. *Ib. id.* 1–50; sold as last (10*s*.). [A critical edition from the original manuscripts (three, here all reproduced in facsimile), with commentary and identifications, and notes on place-names by Sir Ifor Williams].

PRESENT-DAY INDIAN PAINTED POTTERY, by D. H. GORDON. *Journ. Indian Anthr. Inst.*, NS. II, 1948, 9–18.

DAS GRABHÜGELFELD VON BELL IM HUNSRÜCK, by WALTER REST. *Bonner Jahrbucher*, vol. 148, 1948, 133–89. [Early La Tène chariot-burial; the careful excavation revealed other interesting details of burial-rites].

WHITE HORSES AND OTHER HILL FIGURES, by MORRIS MARPLES. Country Life Ltd., 1949. 20*s*. [A full account of them all; likely to remain the standard reference-book on the subject].

A CORPUS OF EARLY ANGLO-SAXON GREAT SQUARE-HEADED BROOCHES, by E. T. LEEDS. Oxford, 1949. 30*s*.

THE ARCHAEOLOGY OF PALESTINE, by W. F. ALBRIGHT. Pelican Books, 1949. 2*s* 6*d*. [A popular but authoritative account by a well-known expert].

LATE SAXON AND VIKING ART, by T. D. KENDRICK. Methuen, 1949. 32*s* 6*d*.

THE ARCHAEOLOGY OF THE CAMBRIDGE REGION, by CYRIL FOX. Camb. U. P., 1949. 37*s* 6*d*. [Second impression, with additions, of this well-known classic, first published in 1923].

AMERICAN QUILTS AND COVERLETS, by FLORENCE PETO. Max Parrish, London, 1949. 15*s*. [An attractive and well-illustrated account of what is curiously, but correctly, described as 'a charming native art'].

ARRETINISCHE RELIEFKERAMIK, by HANS DRAGENDORFF (edited, after the author's death, by Carl Watzinger). Gryphus-Verlag, Reutlingen, 1948. [A profusely illustrated text-book by the leading authority].

Notes and News

GOLD COAST COPIES OF 5TH–7TH CENTURY BRONZE LAMPS

In 1939 the late Captain R. P. Wild of the Gold Coast and Cheltenham showed me a photograph of two bronze lamps which he had bought on 4.7.36 from Kwabena Bonda in Tarkwa, Gold Coast Territory, and which were said to have come from old graves at Attabubu in Bron country, north of Kumasi. This photograph (PLATE, FIGS. 2 and 3) is now published with the permission of the Trustees of the British Museum, to which the lamps were presented by Captain Wild. They are in the Department of Ethnography, their accession numbers being 1936–10–22–5 and 1936–10–22–6. When I pointed out to Captain Wild that these lamps must be degenerate copies of bronze lamps which were used in the Christian east and particularly in Egypt in the 5th–7th centuries A.D., he asked me to publish them, and this would have been done before if the war had not broken out immediately afterwards.

A magnificent example of a hanging bronze lamp in the form of a bird (? dove) was found in an X-Group grave at Firka in the Wadi Halfa district of the Anglo-Egyptian Sudan. It is published in the *Journal of Egyptian Archaeology*, vol. XXI, pp. 197 ff. and in *Oxford University Excavations at Firka* by L. P. Kirwan (London, 1939), Pl. VII and p. 31, and is now on exhibit in the Ashmolean Museum, and a photograph of it (PLATE, FIG. 1) is published here with the permission of the Visitors of the Museum.

Other examples of hanging lamps from the Christian east are given in O. Wulff, *Altchristliche und Mittelalterliche Byzantinische und Italienische Bildwerke*, Teil 1 (Berlin 1909), Plate XXXVI, Nos. 773–4, in O. M. Dalton, *Catalogue of the Early Christian Antiquities in the British Museum* (London, 1901), Plate XXVII, No. 512, and in Josef Strzygowski, *Koptische Kunst*, Catalogue générale des antiq. Egypt. du Musée du Caire, 1904, Plate XXXIII, Nos. 9139, 9141.

In a neighbouring grave to the one in which the hanging lamp (PLATE, FIG. 1) was found at Firka, was a bronze lamp on a stand (A 14/7), which is now in the Khartoum Museum (PLATE, FIG. 4). It belongs to a class of bronze lamps on tall stands, usually described as pricket stands, which like the hanging lamps are characteristic of the Christian east and particularly of Egypt in the 5th-7th centuries A.D. It is published in the *J.E.A.*, loc. cit., Plate XXIII, fig. 8 and Kirwan, loc. cit., Plate X, 2. Other examples of these lamps are shown in Wulff, loc. cit., Plates XLVIII and L, and Dalton, loc. cit., Plate XXVI, Nos. 495–6.

If the hanging lamps from Firka and the Gold Coast are compared, it will be seen, once the rather bovine look of the latter is discounted, that, in addition to both being of similar size, they are both suspended by chains of similar length which meet in a ring. The hinges for the lid of the opening by which the Firka lamp was filled with oil, and the support on which in addition to the bird's two feet the lamp stood when taken down for cleaning and filling, are both copied unintelligently in the Gold Coast example. In fact, until the Firka lamp is seen, the projecting spike at the tail end of the Gold Coast lamp is incomprehensible. While the feathers of the bird are clearly represented on the wing of the Firka lamp, the Gold Coast example is covered with crescentic incisions which look as if the Ashanti caster thought he was representing the hair of an animal, but

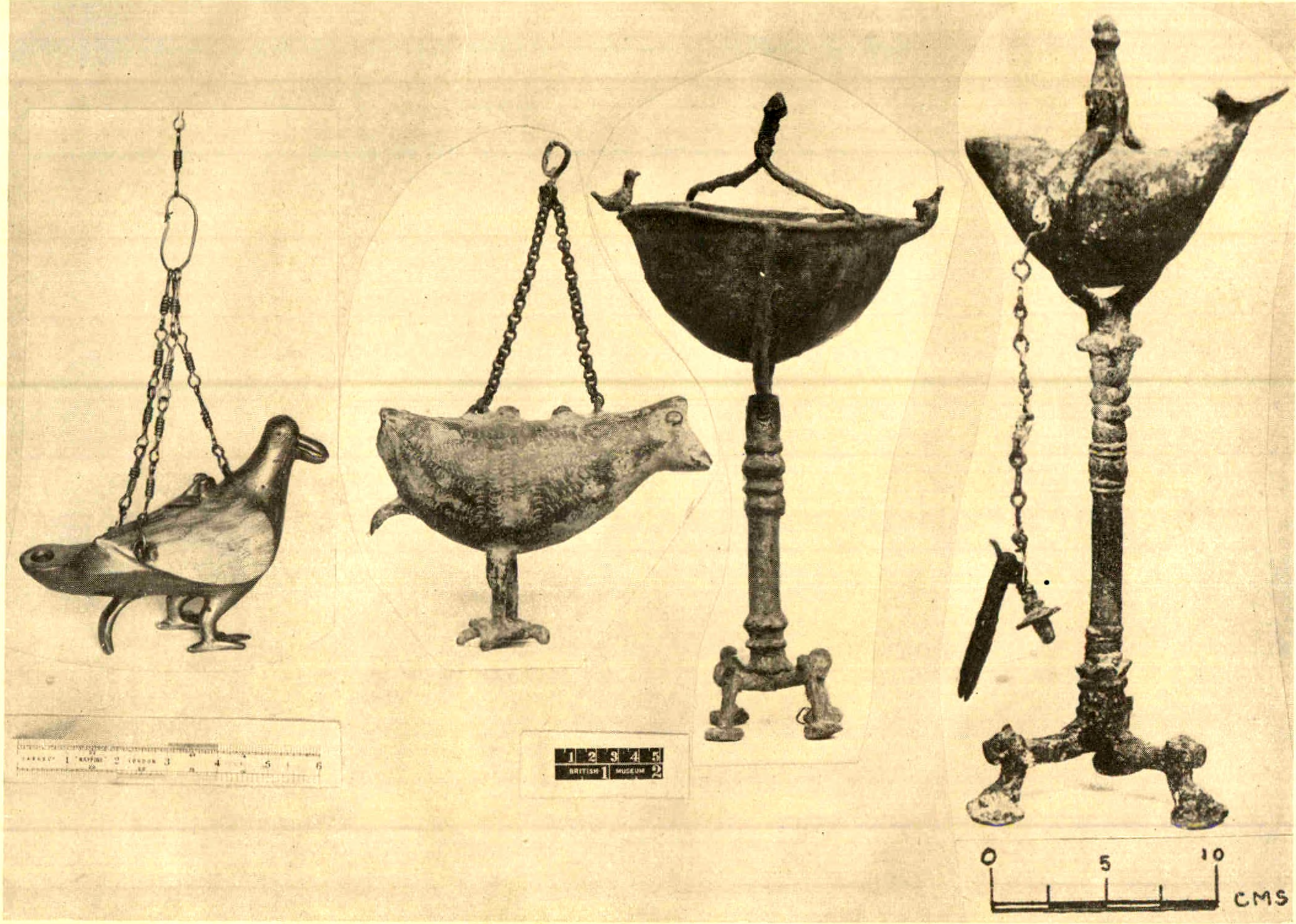
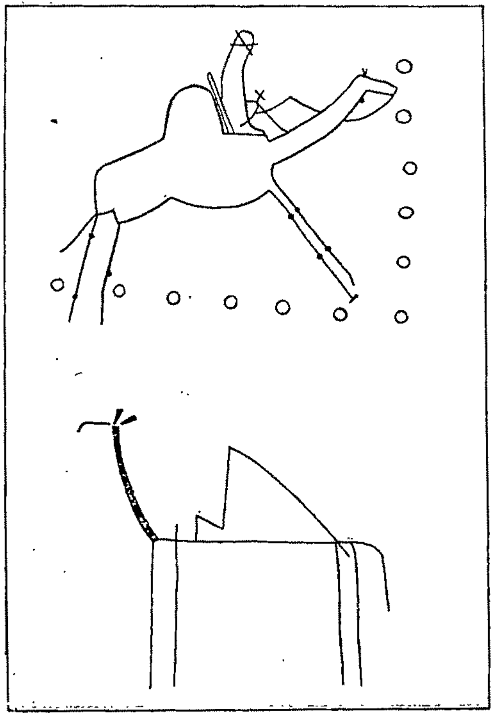


Fig. 1 Fig. 2 Fig. 3 Fig. 4

TWO BRONZE LAMPS FROM THE GOLD COAST (Figs. 2 and 3) AND THEIR PROTOTYPES FROM THE NILE VALLEY (Figs. 1 and 4

TUAREG DRAWINGS ON CRASHED BOMBER*

The accompanying drawings are copies by Mr D. Stenning and myself from the inside port engine fuselage of a crashed Boston on the landing strip at Tamanrasset, in the heart of the Hoggar mountains (22° N. 4° E.) approximately 1000 km. south of Algiers. Tamanrasset acts as a centre for the Kel Ahaggar Tuareg and for transport travelling north or south. These drawings were done in pencil and were amongst many all over the fuselage including Arabic and Tifinagh names and inscriptions and human figures and camel riders. The aircraft crashed at Tamanrasset during the war and all the drawings have been made within the last six years. The artists were either Tuareg or from the mixed semi-negroid population of Tamanrasset, but in view of the subject matter more likely the former. This tradition of drawing camels and riders is an old one in the district going back to the Libyco-Berber rock carvings. The carvings and paintings of lions, elephants and hunters at Oued Mertoutek in the same region are undoubtedly earlier still.



Modern Tuareg artists have an interesting habit of painting in oil, drawing in pencil or scratching the outline of their subject close to or immediately on top of the rock carvings of their predecessors, in spite of the fact that there is room enough to spare on the boulder or stone slab. The subject matter is invariably a camel rider, camel or crude human figure often in conjunction with a Tifinagh inscription. The reproductions here produced are quarter actual size. As for style, significance or similarity to any other primitive art style, past or present, I leave the drawings to speak for themselves. Some of the drawings were published in the *Daily Telegraph*, 26 Sept., 1949. H. T. NORRIS

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* Pictures in caves and on rocks are notoriously difficult to date; they may often be 10,000 or 10 years old. The point of the present note is that the pictures (quite comparable with many much older examples) *can* be dated—they are post-bomber. EDITOR.

INTERNATIONAL CONGRESS, ZÜRICH

Readers of this journal will remember, from the note by Professor C. F. C. Hawkes which appeared in it in 1948 (No. 87 September, pp. 158–9), that the International Congress of Prehistoric and Protohistoric Sciences is the accepted organ of international co-operation in prehistoric and early historic archaeology and related studies, and that at the 1948 Meeting of its Permanent Council the Hungarian representatives asked for, and by unanimous vote received, the acceptance of their Government's invitation to hold the next Session of the Congress in 1949 at Budapest. Last winter, however, this invitation was withdrawn.

It has now been definitely announced that this Third Session will instead be held at Zürich, from 14 to 19 August 1950, by invitation of the Swiss Government and under the Presidency of Professor Emil Vogt. A provisional programme has already been prepared, showing that the week's Session will be followed by excursions to various parts of Switzerland,* and a fuller announcement may be looked for in our next number. The British Members of the Permanent Council are Professors Gordon Childe and Hawkes, the National Secretaries, Professor Stuart Piggott and Dr J. G. D. Clark; the Irish, respectively Professor S. P. Ó Ríordaín and Dr Joseph Raftery.

EARLY MAN IN N. AMERICA

In 1938 an archaeological survey of the Manitoulin District of Ontario was begun by the Division of the Great Lakes of the University of Michigan Museum of Anthropology. It has been continued each season since then, though the past ten years have been spent mainly in the vicinity of Killarney near the eastern end of the area. Here there are three sites that are contemporaneous with the raised beaches on which they lie, and there is complete chronological and industrial sequence.

Each of the two lower sites registers two successive occupations and the materials of the earlier one in each case lie in beach gravels and are rolled. The situation on the other site, George Lake 1, is more complex. The elevation above Lake Huron is about 300 feet, and cultural materials lie on the surface of the beach and to a depth of 15 inches. Most of the artifacts are rolled, some extremely so. The site is primarily a workshop, owing its location mainly to a small area of white uniformly textured quartzite in the barren rocky hills of the southern escarpment of the Laurentian highlands. All implements are of that material. The distinctive type is a large flaked semi-lunar blade ranging in length from five to eight inches. The most common implement is a large ovate blade, coarsely flaked in most cases, and thick. Only two hafted points have been found, one of which is a small side-notched projectile point. The other is of Yuma type, with the tang nearly as wide as the blade. Some 200 implements and about thirty thousand flakes have been collected.

George Lake 1 is about thirty acres in extent, with the main concentration of cultural material between 280 and 300 feet above Lake Huron, around the north end of a small lake half a mile long. Much material however is found at lower levels down to the shore of the small lake, at 209 feet elevation, and a few flakes have been dredged from the lake itself near the shore. This material was brought to its position either by surface drainage, or the following of the receding water-level by the inhabitants of the site; or perhaps a combination of both. Excavations so far have been confined to a well-formed breach of gravel about 500 feet long. George Lake 1 is non-ceramic, and bone of any kind is absent (see 'An Early Industry on a Raised Beach, near Killarney, Ontario'. By E. F. Greenman, *American Antiquity*, VIII, No. 3 (1943): 260–65). The intermediate site, Chikanising Creek 1, has an elevation above Lake Huron of 56 feet, and is also non-ceramic. The lowest site, Killarney Bay 1, is at 28 feet elevation, and has pottery, copper, ground stone including bored slate bracers and gorgets, flint projectile points and scrapers, and burial mounds of earth. The earlier occupation at the intermediate site, at depth in the beach gravels, appears to be more closely related to George Lake 1 than to Killarney Bay 1. There are halves of badly rolled semi-lunar blades and tanged points of quartzite, the latter again recalling Yuma types.

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* Basel, the Jura, and Neuchâtel; St. Gallen and Wildkirchli; Graubünden and the Valais.

The dating of these sites involves the post-Algonquin changes in Great Lakes water-levels through isostasy and drainage. Lake Algonquin was the last of nine glacial Great Lakes formed by the retreat of the ice during the decline of the Wisconsin glacier some 25,000 to 35,000 years ago, and the beach of George Lake 1 was built as the waters receded from the Lake Algonquin shore due to the tilting upwards of the Great Lakes basins north of Saginaw Bay. The actual figures in years are approximations based upon the elevations of the three sites above present lake level, that is, upon the extent of upward deformation of the land since they were functional beaches. Nipissing Great Lakes, the stage immediately preceding the present, came to an end with the abandonment of the outlet into the Ottawa river valley at North Bay, Ontario, and the return of the discharge of the upper lake basins to Port Huron, Michigan. The increased volume of water through Niagara then began the cutting of the Upper Great Gorge of that River, a process which is estimated to have taken 4000 years (See Late Quaternary Upwarpings of Northeastern North America, by Ernst Antevs, *The Journal of Geology*, XLVII, No. 7 (1939): 720). During that period tilting deformed the basin of Lake Huron to an extent producing an elevation of 85 feet at Killarney. It accordingly took about 1300 years to produce the 28 feet of elevation of the Killarney Bay site 1, and about 2600 years for the 56 feet of elevation of Chikanasing Creek 1. The rate of tilt since Nipissing Great Lakes has been about two feet per century. If this is applied to the 300 feet elevation of George Lake 1, a lapse of about 15,000 years is indicated since the formation of that beach and the deposition of cultural materials on and beneath its surface. However, the subsidence of the water-plane from the Lake Algonquin shore was not, as was formerly believed, one continuous movement in the same direction (see in this connexion, 'The Submerged Valley through Mackinac Straits', by George M. Stanley, *The Journal of Geology*, XLVI, No. 7 (1938): 972). There is, further, no certainty that the post-Nipissing rate of tilt was the same as that before Nipissing Great Lakes. George Lake 1 may be older or more recent than the 15,000 years antiquity suggested by the data. Its closest cultural connexions, aside from the possibility of Yuma, are with three sites at North Cape, Norway, on raised marine beaches with elevations between 24 and 30 metres, the Nordskog, Kongshofmark and Hjemmeluft sites (*Stone Age Finds in Finmark*, by A. Nummedal, Instituttet for Sammenlignende Kulturforskning, Oslo, 1929).

Complete excavation of all three sites will take at least six more seasons. Work on George Lake 1 is necessarily slow because of the inaccessibility of the site, and the difficulties of making a contour map and grid plan in thick bush. The work is done by students from various universities, who receive instruction and credit in the University of Michigan in the Department of Anthropology.

E. F. GREENMAN

Ann Arbor, Michigan, *September* 27, 1949.

THE DATE OF CAMLANN

King Arthur's last battle is recorded tersely in the *Annales Cambriae* : 'The action of Camlann in which Arthur and Medraut fell together. And mortality in Britain and in Ireland (1)'. This is usually dated *c.* 537–9. But the same authority places the battle of Mount Badon (*Mons Badonicus*) just 21 years earlier, so, if the two entries hang together, and Badon (as is frequently argued) should be dated *c.* 500, then the date implied for Camlann would be somewhere near 520.

In this connexion, the second entence quoted is worth noting. It records a pestilence affecting both Britain and Ireland. This can hardly be the famous 'Yellow Plague' of *c.* 549, which appears in its proper place a decade later. This led to search in the Irish Annalists—at this period much more detailed than any Welsh or English chronicle—and discovery of the following entries in the *Annales Tigernachi* (2) :—

'K. VI (i.e. New Year's Day on a Friday. This would point to 538. But we should probably read .V. (537) which would fit the reference to the death of Pope Silverius). P(er)dit(i)on panis. Silveri(us) Pap(a) Ro(m)an(us) r(egnavit) an(no) .I. m(ens)ib(us) .V. d(iebus) .XI. q(u)i (con)fesu(r)is obuit.

Manchan Maetha cecidit.

Kl. VII (read VI). Nat(iv)itas G(r)igorii Papa. Nem ep (iscopus) obiit.

Vigil(ius) Pap(a) nat(i)one Ro(m)an(us) r(egnavit) an(nus) .XVIII. m(en)sib(us) .VI. d(iebus) .XV., q(u)i Siraciusis defunct(us) via Salaria sepult(us) (est) ut Beda boath. K. I. (New Year falls on Sunday) *Mortalitas magna* q(uae) Blefed d(icitur) in q(u)a MoBi clairi (n)each c(ui) no(men) Berchan, Brecano, p(r)o(f)eta p(er)iit'.

The year of the 'mortality called Blefed', coming one year after the birth of Pope Gregory, and two years after the death of Pope Silverius, clearly represents A.D. 539 or 540. The obit of St. MoBi appears in the Ulster Annals under 544, but this obviously belongs with several nearby entries which are post-dated several years. The *Chronicon Scotorum* gives the date as 541, but it dates the Yellow Plague 551, at least two years late. There is, therefore, no reason whatever to question the correctness of Tigernach's date for the *Blefed.*

No other pestilence is mentioned between 500 and 539. The Blefed must be the pestilence mentioned by the *Annales Cambriae* along with Camlann. This would point to Camlann being fought in 539, Badon in 518. There is, however, a weak link. We cannot be sure that the compiler of the *Annales* did not blunder in assembling his entries from disparate sources.

P. K. Johnstone.

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[1] An(no) XCIII. In *Y Cymmrodor*, IX, 141.

[2] ed. Whitley in Stokes, *Revue Celtique*, 17 (1896), pp. 136–7.

Reviews

LA MOSQUÉE OMAYYADE DE MÉDINE; ÉTUDE SUR LES ORIGINES ARCHITECTURALES DE LA MOSQUÉE ET DE LA BASILIQUE. *By* J. SAUVAGET. *Publ. for the Institut français de Damas by Vanoest, les Editions d'Art et d'Histoire, 3 et 5 rue du Petit Pont, Paris, 1947, quarto, 199 pp., 6 plates and 37 figures (price not stated).*

With this volume Prof. Sauvaget adds an important contribution to his studies on Umayyad monuments. It reproduces, without alteration, a course of lectures delivered at the *Ecole des Hautes Etudes* and falls into two parts: (*a*) an attempt at the reconstruction of the Umayyad mosque of Medina and (*b*) the wider problem of the architectural origins of the mosque in general and of its model—the basilica.

The first mosque of Islam—the house of the Prophet Muhammad at Medina—has always enjoyed a privileged position. It was enlarged by the orthodox caliphs, Umar and 'U*th*mān, and completely rebuilt at the beginning of the 8th century A.D. by the Umayyad caliph Walīd ibn 'Abdulmalik. The edifice has suffered so many alterations, additions and restorations in later years, that we must rely almost exclusively on literary sources in any attempt to reconstruct it. This Sauvaget accomplishes with consummate skill. He proceeds first to analyse the texts listed by Prof. K. A. C. Creswell in the first volume of his Early Muslim Architecture (Oxford, 1932) and to check their reliability. As is often the case with Arabic texts, copied with more or less accuracy by generation after generation of compilers, only a limited number of original contributions and eye-witness accounts emerges. Sauvaget singles Samhūdī out for special attention. This author of a detailed history of Medina died in A.D. 1506 and would not, at first glance, qualify as an authority for the reconstruction of an 8th century mosque; but the value of this work remains unequalled. It was written not only to coincide with an important restoration of the monument, but was intended to call the attention of Samhūdī's contemporaries to the faults in earlier restorations. Samhūdī collected his material during a sojourn of forty years in Medina. His book represents the fruits of minute investigation and careful observation. By comparing Samhūdī's detailed and conscientious description with that of earlier sources, Sauvaget is able to conclude that the plan of the Umayyad mosque had survived without major alterations until 1481; but that the old decorations had completely disappeared in the fire of 1256. This enables him to draw up an orthogonal plan of the mosque.

Sauvaget further distinguishes two main types among the Umayyad mosques known to us:

(*a*) That represented by the older monuments, Basra A.D. 655, Kufa A.D. 670, consisting of a flat-roofed edifice supported by equidistant supports, and adjoining courtyard flanked by porticoes.

(*b*) The 'Syrian type', mosque distinguished by an elevated central aisle, roofs resting on colonnades, adjoining courtyard flanked by porticoes of varying depth as well as by one or more minarets.

The mosque of Medina cannot, conveniently, be placed in either of these categories. The porticoes are not symmetrical. The equidistant supports in the main body of the edifice resemble those of Basra, but the cupola in front of the mihrab and the sumptuously decorated transept remind us of the Syrian style. According to Sauvaget, the mosque of Medina represents a compromise. Walīd intended to preserve the main features of the mosque in which the Prophet had performed his devotions; but the central aisle, the niche-shaped mihrab and the minarets were Umayyad innovations. The importance given to the central aisle for the first time at Medina was to become the essential feature of the Syrian mosque.

The second half of Sauvaget's book is devoted to the origin of the mosque in general. The plans of early mosques were based on those of basilicas. So were, Sauvaget points out, the audience halls of the Umayyad palaces (M*sh*atta, *Kh*irbet Minye, etc. . . .). Few of these halls have survived destruction, as they were normally placed on the first floor of buildings which have crumbled.

With the aid of a great number of historical and literary texts, Sauvaget illustrates the functions of the mosque and the ceremonies performed in it. 'The mosque is not so much', he says, 'an autonomous edifice for the cult as *an annex to the palace of the caliph or to the residence of the governor*, in a provincial town'. Following a tradition established by Muhammad himself, it was reserved for all purposes for which the private reception rooms at the palace were unsuitable. 'We must, therefore', he concludes, 'look for the key to the architectural lay-out of the mosque not among liturgical but among ceremonial, governmental institutions'. He would have us go further and suggests that, from the architectural point of view, the difference between mosque and audience hall lies only in the size of the terminal exedra, large and open as in the apse, or reduced to the narrow dimensions of the mihrab. This leads him to look for the model of the mosque, not, as had been done hitherto, in the domain of religious architecture, but among palatine monuments. A parallel development seems to have taken place in Christian architecture; the basilica, like the mosque, was inspired by the plan of the audience chamber. A concise statement illustrating the truth of this argument, Sauvaget finds in the inscription placed on the triumphal arch of St. Peter's in Rome by Constantine.

> Quod duce te mundus surrexit in astra triumphans,
> hanc Constantinus victor tibi condidit aulam.

Next to the inscription is an image of the Emperor holding a model of the church. *Aula* here, Sauvaget affirms, has the narrowest meaning of 'sovereign court', 'palace'.

Some may find it difficult to accept all the arguments so eloquently presented in the second half of the book, but the first, dealing most skilfully with the reconstruction of the 8th century mosque, could not be improved upon in the absence of scientific excavations. It is hoped that wider circles of readers than those particularly interested in Muslim art and archaeology will find matter for thought in Prof. Sauvaget's stimulating and absorbing book. D. S. RICE.

MEN OF TASTE. *By* MARTIN S. BRIGGS. *pp.* VIII, 232; *index, coloured frontispiece and* 52 *photogravure illustrations. Batsford,* 1947. 15*s net.*

In his title 'Men of Taste' Mr Briggs refers to the patrons of art through the ages and he sets out to trace the influence of these 'supermen', as he calls them, upon the development of art and upon the lives of the working artists whom they employed. To develop such a theme fully, even in a limited field, would be a difficult task and no very systematic treatment can be expected in a book that ranges from Rameses II to John Ruskin and William Morris. Mr Briggs is mainly interested in architecure and most of his admirable illustrations are of architectural subjects. The remainder are mostly reproductions of sculptural or pictorial portraits of the subjects of his chapters, such as the fine portrait of Cardinal Richelieu by Philippe de Champaigne, the well-known bust of Pericles or Sir Thomas Lawrence's George IV. The frontispiece is a fine coloured reproduction of the painting of Francis I and Margarete de Navarre by R P. Bonington now in the Wallace Collection. The chapters on the worthies that Mr Briggs selects for treatment are discursive and anecdotal accounts of the men and their activities as patrons of artists and architects. Mr Briggs might have been wiser to start his survey at a later period. In the earlier chapters on Rameses, Pericles and Hadrian suitable material for his method is scanty and his summary statements on conditions in the ancient world are sometimes misleading. In the later periods he finds richer veins and offers much that is lively and interesting, such as the entry in the diary of John Evelyn for January 10, 1662—' Being called into his Majesty's closet when Mr Cooper, the rare limner, was crayoning of the King's face and head, to make the stamps for the new milled money now contriving, I had the honour to hold the candle whilst it was doing, he choosing the night and candlelight for the better finding out the shadows. During this his Majesty discoursed with me on several things relating to painting and graving '.

' Men of Taste ', including the elegant jacket designed by Felix Kelly, is itself a tasteful production and will give pleasure and entertainment to amateurs of architecture and the fine arts. G.F.F.

STATUES ON COINS. *By* PHYLLIS WILLIAMS LEHMANN. *pp.* x 72, *with* 51 *photographic plates, illustrations in the text and indexes ; H. Bittner & Co. New York,* 1947. $3.50.

Students of Greek sculpture have often turned to coins for aid in the solution of their problems and specific coin types have been related to actual statues. Whereas, however, it is agreed that many Hellenistic and Roman coins reproduce known statues the view of Percy Gardner that this practice was unknown to the die-cutter of classical times has been accepted with few dissentients. In this thorough and scholarly work Miss Lehmann takes up the problem again, and, by a study of the types of selected coins of cities of Southern Italy and Sicily, endeavours to prove, that these coin types ' reproduce famous lost statues and constitute focal points about which numerous statues and statuettes may be grouped '.

All the coins considered were struck between 480 and 323 B.C. and Miss Lehmann finds within that period, among those coin types ' which, from a purely visual standpoint, give the impression of reproducing full length statues ', ten examples which are also unique. She argues that ' if a statue of unknown provenance is iconographically and stylistically identical with a coin type, and that type is unique, struck at a specific time by one, and only one, city, it is logical to assume that statue and coin type alike depend upon a common archetype. Such a coin type not only constitutes a reproduction of a given statue ; by virtue of its own unquestionable provenance, it affords the means of attributing closely related sculptures to a given region, hence, of enlarging the number of works which may reasonably be considered products of that region '.

It may be doubted whether the logic is entirely watertight, but when Miss Lehmann goes on to the next step, ' to examine whatever Greek and Roman sculptures were available in published form, from small bronzes and terracottas to monumental sculptures, to see whether any figures iconographically related to those numismatic types could be found ', some interesting conclusions emerge. Thus the celebrated bronze statue found in a house on the Via dell' Abondanza in Pompeii in 1925 and variously explained as a victorious young athlete pouring a libation, a lychnophoros adapted from such an earlier type, or a Ganymede, is linked with the standing figure on the reverse of the didrachm issued by Selinus between 466 and 409 B.C. On stylistic ground the Pompeii youth has been vary variously attributed ' now to Pheidias or his circle, now to Alkamenes, now to an Argive sphere or again, a neo-classical Athenian of the first century B.C. ' Miss Lehmann examines and compares the figures in detail and concludes that ' given this remarkable degree of typological and stylistic similarity, it is reasonable to assume that statue and coin type alike reflect a common archetype—the original cult statue of the river god Hypsas . . . Judging from the didrachm the archetype must have been made about 450 B.C. '

By similar arguments the famous Idolino is brought into relation with the figure on the reverse of the stater of Pandosia in Bruttium issued at a date somewhere around 430 B.C. Both statue and coin type would ' reproduce a lost statue of the river god Krathis, whose waters were famous in antiquity for their magical powers '.

Still more conjectural is the attempt to associate the bronze statuette from Herculaneum, originally identified as Seleucos Nikator and now generally accepted as Demetrios Poliorketes, with the figure of the river god Krimisos on the obverse of the tetradrachm issued by Segesta in the latter part of the fifth century B.C. The unusual pose of the figure, with one foot raised and resting on a rock, is similar to that of the coin type but in reverse. Apart from this general resemblance there is little reason to regard this statuette as a remote descendant of a lost cult image of a river god that once stood in Segesta. Although there is nothing inherently improbable in the representation of Demetrios in the guise of a young hunter-river god, the existence of such a cult image is a conjecture, for which the author can find little evidence and for which she claims no more than probability in this instance.

Miss Lehmann also discusses Apollo figures in relation to coins of Metapontum and seated and standing figures of Herakles with reference to coins of Croton, Thermae Himerenses and Herakleia, and on Plate XV reproduces fourteen further coins, whose types are definitely suggestive of sculptural archetypes. Miss Lehmann's conclusions must await the verdict of specialists but her excellent monograph opens up some very interesting lines of enquiry. It is admirably produced and illustrated. G.F.F.

PREHISTORIC CERAMIC STYLES OF LOWLAND SOUTH AMERICA, THEIR DISTRIBUTION AND HISTORY. *By* GEORGE D. HOWARD. *pp.* 95. 15 *figs.*, 15 *pls. Yale University Publications in Anthropology, no.* 37. *Yale University Press* (*London : Geoffrey Cumberlege*), 1947. 8*s* 6*d*.

The appearance of this paper raised hopes that new data had been found which would at last link up the scattered archaeological discoveries of South America east of the Andes. It is a praiseworthy attempt at synthesis, and some rather inaccessible material has been gathered and well illustrated, but it cannot be said that any very notable advance in knowledge has been made. It is useful to have illustrations and descriptions of most of the known Lowland South American pottery styles set out in one small volume, but it must be realized that there are obvious omissions. The El Cerrillo style of the Parana River is very inadequately illustrated, in that the modelled decorations, which are a prominent feature of it, are barely mentioned, though they have been figured recently by Aparicio, and over thirty years ago by Joyce. A peculiar style of burial urn from Pará, figured by Joyce, is not mentioned at all, although it must represent something completely different from the neighbouring site of Marajó.

The results are analysed in the excessively mechanical manner of which the Yale School has already published various examples. A prominent feature of this is the making of long lists of traits of very unequal value for comparison, some of which are for all practical purposes repetitions of the same thing in different words, for example it is hardly significant to mention monochrome painting in addition to red paint as a 'diagnostic trait' of a particular 'complex'. The latter term, which is defined as a characteristic grouping of traits within a style, lends itself to similar misuse. Thus, to say that the Napo Anthropomorphic Complex occurs in the Maracá style as well as in a number of others (bottom of page 56) is to use a form of words to disguise obvious differences. The acme of meaningless verbiage seems to be reached in the definition of the Mound Hernmark Painted Complex, whose 'diagnostic traits' include 'plain or painted, monochrome or polychrome painting' (footnote page 70). The impression given by the work in general is that such sound conclusions as the sketchy evidence allows have been reached in spite of the method.

It is gratifying to English readers that the work of T. A. Joyce, published as long ago as 1912, can still be quoted as essentially sound, but it appears that it was even better than the author appreciates. He says that it is evident that Joyce's Southern Complex actually include two unrelated styles, without realising that Joyce himself said that 'the peculiar style of pottery in the neighbourhood of the Parana', meaning what is now called the El Cerrillo style, implied just such a division.

A paper dealing with the distribution of styles deserves a better map; it should not have been difficult to get an outline map whose graticule was not so close as to make it look like coarse wire gauze. Finally, the expression 'biomorphic face' creeps in twice on page 65. Where outside the Department of Anthropology at Yale do they have non-biomorphic faces?

G. H. S. Bushnell.

THE ANCIENT MAYA. *By* S. G. Morley. *pp* xxxii+520, 95 *pls.*, 57 *figs.*, 12 *tables.*

Anything written about the Maya by Dr Morley will be read with attention and pleasure by students of American archaeology, and this book, in which he sums up a lifetime of experience, will refresh their memories on many points and enlighten them on some. It contains a good account of most sides of Maya life and work, presented in a readable form. It is particularly useful to have Maya history set out, with convenient tables, in terms of the Goodman-Martinez-Thompson correlation of dates with the Gregorian calendar. Perhaps the best part of the book is the emphasis laid on the continuity of many features of Maya life. It is most illuminating to read Dr Morley's account of Maya agriculture, showing how little it has changed in method from the earliest times to the present, and of the part it probably played in the abandonment of the Old Empire cities. A pleasing human touch is given by the story of the Xiu family, simple peasant farmers directly descended from the New Empire rulers of the great city of Uxmal, with an intermediate stage when they were ennobled by the Spaniards. It is good to have a lucid account of the relationship between the Old and New Empires. This was not, as is still commonly supposed, a wholesale migration from the old cities of the south to virgin territory in northern Yucatan, but rather a more intensive development of a region which had been occupied from very early times, after the abandonment of the southern area.

In spite of these and other useful features, the book is more for popular consumption than for the specialist. Much important recent work bearing on the origin of Maya culture, for instance the Smithsonian expeditions to Tres Zapotes and other sites in Vera Cruz, is dismissed with little more than a statement that the author does not agree with the conclusions. He pays lip service to the importance of pottery in archaeology, but the unbalanced nature of his choice of illustrations renders much of his necessarily brief account of Maya pottery of little value. Apart from some pictorial scenes from Old Empire painted vessels, only three plates and a text figure are devoted to pottery, and these are all taken up by New Empire types, comprising four examples of Plumbate

Ware, three of Fine Orange, four of Yucatan slate and four other objects, an altogether disproportionate distribution. A better idea of the older types of pottery may be derived from a recent popular article by Dr Morley in the periodical *Life !*

Dr Morley's rather exclusive preoccupation with the Maya leads him to ascribe, tentatively it is true, comparatively remote dates to their early stages. 'A thousand years or more' B.C. for the introduction of agriculture to the lowland Maya area seems an over-long time before the estimate of about A.D. 1 which is widely accepted for its introduction into the Valley of Mexico. Perhaps the truth lies between the two. He discusses and rejects the recent conclusions of botanists who believe that maize was originally cultivated in South America, and prefers to stick to the 'teosinte' theory of its origin, with Guatemala as the place. The last word has not been said on this subject, and the possibility remains that agriculture may have arisen in more than one part of America, and that maize was not the first plant in every case.

Some minor errors have been noticed. An interval between two attempts at conquest is not an interregnum (p. 103). The fraction given as 53,059/100,000 on p. 306 should read 3,059/100,000. The use of the word 'monastery' on p. 348 can not have been intended, since there is no evidence that the buildings called monasteries by the Spaniards were used as such by the Maya. On p. 388 it is stated that calcite temper practically replaces sherd temper in utility ware in Chicanel times, whereas on p. 389 the temper of such ware is said to remain the same throughout Mamom and Chicanel times and later, and to be sherd. On p. 451, it is implied that the finest Peruvian textiles were those of Nazca and that they were of vicuña wool ; it has long been known that most of the fine textiles formerly ascribed to Nazca came from Paracas, and that wool embroidery (not necessarily vicuña) on a cotton foundation is commoner than wool weaving throughout.

The production of the book is, on the whole, pleasing and a topical touch is given by the chapter numbers, which appear on each page in the Maya bar-and-dot notation, as well as by the date of publication, 1946, which is shown similarly at the beginning. The text figures and plans are excellent, but many of the very numerous half-tone plates have suffered much in reproduction and the detail is woolly and blurred. In particular, the air photograph of Coba, plate 38(b) is worthless and one suspects that the original was none too clear. Plate 64, showing reconstructions of four steps in making a stela by a modern artist, does not inspire confidence, and the drawing showing its erection appears mechanically impossible.

The final appraisal of the Maya civilization leads Dr Morley to 'acclaim the ancient Maya, without fear of successful contradiction, as the most brilliant aboriginal people on this planet'. This is a large claim and will doubtless be hotly contested by others who have their pet aboriginal races, but it is a measure of the author's enthusiasm, and the reader will enjoy the opportunity of judging its merits for himself.

G. H. S. BUSHNELL.

THE ATTALIDS OF PERGAMON. *By* ESTHER V. HANSEN. *pp.* XXXVI, 464 : *index, bibliography and map* : Vol. XXIX of Cornell Studies in Classical Philology. *Cornell University Press, Ithaca, New York*, 1947. $4.50.

The little kingdom of Pergamon in the valley of the Caicus in the north of Asia Minor lasted for less than two hundred years from the time when it emerged under Philetaerus at the beginning of the third century B.C. until it was bequeathed to Rome by the testament of Attalus III in 133 B.C., to form a few years later, after the defeat of the pretender Aristonicus, a part of the Roman province of Asia. During this short period

It played a notable part in the general history of its time. Attalus I did good service to the Greek cause by his victories over the Gauls, which earned him the title of ' Soter '. By their military successes and their shrewd diplomacy the adroit monarchs of Pergamon secured their position against the more powerful successor states. Above all by their discernment in recognizing the power of Rome they enlarged their boundaries and built up their prosperity. It has been plausibly suggested that the Roman campaign against Philip was actually prompted by Attalus, who skilfully exaggerated the importance of a pact between Philip and Antiochus and played upon Roman fears of an imaginary ' Seleucid peril '. However this may be, the contribution of Pergamon to the Roman successes in her eastern campaigns secured for her favourable treatment in the final settlements. Her position thus assured, Pergamon developed into an active centre of Hellenism. The ' Attalicis conditionibus ' of Horace recalls the proverbial wealth of her kings, which they used not only in liberal subsidies to Greek cities but also in the adornment of their capital and a generous patronage of the arts, which drew artists and craftsmen to the city from many quarters. It was under Eumenes II that the city was laid out in the magnificent series of rising terraces, adorned with noble buildings, that made it one of the finest examples of civic planning to be found in the ancient world. At successive levels were to be seen the vast marble altar of Zeus with its colonnaded sides, the famous library and, crowning the summit, the palace of the king. The grandeur of the whole design foreshadows the splendours of Imperial Rome, and the great altar of Zeus Soter, adorned with a sculptured frieze representing the battle of the gods and giants, a subject which symbolizes the greatest service to Hellenism of the ruling house in the defeat of the Gauls, was among the most notable monuments of antiquity Its fame is attested by the words in the *Book of Revelations* addressed to the angel of the church of Pergamos, ' I know where thou dwellest, even where Satan's seat is ' ; and the obscure Ampelius in the 2nd or 3rd century of our era noted this altar and its impressive sculpture. Scholars, mathematicians and philosophers were drawn to the library of Pergamon, second only to that of Alexandria. Attalus III, the last of the dynasty, an obscure and enigmatical figure, appears to have retired to his palace to pursue botanical and medical researches and a distorted tradition lingers into the Middle Ages ascribing to him the invention of chess. Professor Hansen cites Chaucer's words in ' The Book of the Duchess '

> Ful craftier to pley she was
> Than Athalus, that made the game
> First of the ches, so was hys name.

References to Pergamon crop up in many quarters, for it has a relevance in several fields, in Roman history, in art, architecture and town-planning, and in the organization of city government and the spread of Hellenism in the East.

In this volume Professor Hansen presents a careful and systematic study of every aspect of Pergamene history. Her narrative is closely documented from a large specialized literature on widely separated themes. After a survey of the factual history of the dynasty a number of special topics are handled, such as the organization of the kingdom, the government of the capital and its sources of revenue and coinage. Topographical and archaeological subjects are treated in chapters on the building activities of the Attalids and the Art of Pergamon, and an appendix contains a list of the coin types of the Pergamene kingdom. Professor Hansen writes for specialists and assumes that they have access to illustrations of the works which she describes. In view of present difficulties in obtaining books a companion volume of plates to illustrate these topics would be very helpful to students.

In her chapter on the Attalid patronage of learning Professor Hansen deals with a number of scholars and literati whose works are for the most part known to us only in fragments or by repute. They include Antigonus of Carystus, sculptor and art critic, an ultimate source for many passages in the books on art in Pliny's Natural History, Crates, literary critic and grammarian and probably head of the Pergamene library, the most important scholar of the reign of Eumenes II. Apollonius, the mathematician, dedicated to Attalus I a portion of his 'Conic Sections', the work which gained for its author the title of 'the great geometer'. The only complete works extant of any Pergamene author are the *Theriaca* and the *Alexipharmaca* of Nicander. They reveal Nicander as a metaphrastes, versifying material drawn from various sources to form a poem of didactic character typical of Alexandrine literature. Quintilian's reference to Virgil as a follower of Nicander has led to much speculation on the extent to which Virgil may have imitated the lost works of Nicander, particularly his *Georgica* and a poem on bee-keeping. Professor Hansen dismisses this claim, but the description of a serpent in the third Georgic closely imitates an extant passage of the *Theriaca* and it is not improbable that the fourth Georgic contains echoes of the lost *Melissourgica.* The influence of another of Nicander's works, the *Heteroioumena*, upon Ovid's Metamorphoses is generally admitted so that Nicander, in spite of his pedestrian qualities, is not an altogether negligible figure in the history of Letters.

The final chapter is concerned with the cults and religious organizations for which evidence can be found in the coins, inscriptions and architectural remains of Pergamon, and with the development of the cult of the Attalids, culminating in the claim of Attalus III to be regarded as the incarnation of a god.

In gathering into a single volume the various strands of Pergamene history Professor Hansen has performed a service for which classical scholars will be grateful

G. F. Forsey.

LES MONUMENTS FUNERAIRES D'ARLON ROMAIN (extrait des Annales de l'Institut archéologique du Luxembourg, LXXVI, 1945). *By* M. E. Mariën. *Reprinted, Brussels*, 157 pp., 54 *figures.*

This is a careful study of the Roman monumental and sculptured fragments from Arlon in Luxembourg, including the material found in 1936 and 1937, illustrated by the author's drawings and restorations. The remains are classified according to the type of monument from which they come, with full use of the comparative material from other sites to place them in their setting.

While Igel and Neumagen have been fully treated, we must await further individual studies of sites, as M. Mariën has recently done for Buzenol, and now for Arlon, before any final settlement of the problem of the origin of the lofty monumental pillared tombs of the Igel type. Meanwhile M. Mariën has summarized the evidence for this type from Igel, Neumagen and Buzenol, while leaving the group from various Moselle sites for the treatment projected by the Römisch-germanische Kommission, and then done a detailed classification of four types exemplified at Arlon.

In subsequent chapters Mariën discusses the simpler tomb, oblong in ground-plan, with decorated sides and back, which developed out of the early military tombstone-slab, secondly the local development of the altar-tomb with its affinities with Pompeii, and finally the very simple Gaulish tombs with the main block hollowed out at the top to make a recess for the ashes, and coped by a capital often half-rounded or triangular, to simulate a house-roof.

He concludes with a valuable chapter on the art of the Arlon monuments and traces its development from the Claudian style, strongly influenced by the official and military style of the Rhineland, to the fully developed civilian tradition of the later second and first half of the 3rd century. The local school does not attain the artistic inspiration of the Neumagen sculptures, but does reveal what could be achieved by local craftsmen working for the merchants and landholders of Orolaunum (Arlon), a prosperous town at the intersection of the roads from Rheims to Treves, and from Metz to Tongres. The reliefs are useful in giving scenes of daily life, of merchants selling cloth, and tenants paying rent, of carriages and country carts, and humble tillage of the fields. Other scenes portray a dinner-party, or perhaps the funeral banquet, but the author reserves for future treatment his main discussion of the symbolism of these monuments. They also yield material for provincial fashions, as do the tombstones at York, to show how far imperial styles of hair-dressing were adopted in these distant provinces, or how the men favoured Gallic forms of dress.

The student both of Roman funeral monuments and of art in the northwest provinces should profit by this and other works promised from the pen of M. Marïën.

R. P. WRIGHT.

CASTE IN INDIA. *By* J. H. HUTTON. *Cambridge University Press*, 1946, *pp.* 279 *and map.* 18*s*.

On reading a book about caste in India, the majority of those who have some knowledge of the subject, and many of those who have not, will judge that some part of this complex problem has received inadequate consideration and feel impelled to add their contribution ; hence the 5000 published works on this subject which the author mentions in his foreword. India is so large, the ramifications of caste so considerable, that it would be hard to say that any one of these works was unjustified. Professor Hutton with a wide experience of the literature of the subject feels that, for the student of anthropology, there is a need for a book on this subject which combines compactness with a good comprehensive survey.

Anyone wishing to be enlightened on the subject of caste in India will look for information about its structure, function, origin and future, how it affects the lives of the inhabitants of India and how it is influenced by the increasing impact of Western material progress. Judged by these standards Prof. Hutton has made a valuable contribution which is clear, readable and gives a satisfying, though in view of what has been said probably not final, exposition of the caste system in India.

In four chapters the racial background is presented with a succinct completeness which might so easily have been a dreary catalogue, but is in fact extremely readable. A few points may however be noted. The indifference of the Koragas of Coorg to caste and the beneficial effect on both the people as individuals and Coorg as a social and economic area is perhaps not so strongly stressed as it should have been. The bulk of the Punjabi Jats, especially from Rohtak and Hissar, and the Jat Sikhs are so totally dissimilar facially, mentally and temperamentally that Sikhism cannot account for the difference ; they must be of different racial stock.

The reliability of the statistics of caste and religion is in some instances and in some districts often questioned. It is contended that Hindu politicians take steps to ensure the inclusion in the census returns as Hindus, of tribal peoples who are not Hindus, do not wish to become Hindus and who are increasingly fixed in their determination not to be submerged as outcasts on the fringes of Hindu society but to retain their independence, individuality and dignity. This is particularly noticeable among the Kols and Hos of Kolhan in Southern Bihar.

In its chapter on structure this book shows that caste is far from simple and clear cut, and those on strictures, sanctions and functions show caste as it affects the lives of millions of caste Hindus. One of the features of the caste system that has always troubled Western observers is the idea that an individual born an outcast must remain an outcast all his life and cannot, at any rate in his own country as a member of the Hindu community ever improve his status. Although Prof. Hutton says: 'It is not so rigid that a caste cannot rise in the social scale', caste does not work out this way in practice, certainly for the individual. Even the education, wealth and influence of the Shaha trading community of Sylhet win them but little real regard, though their eligibility in the marriage market seems to have given them the right to purchase their brides from the higher castes. This right in view however of the pratiloma caste distinction which lowers the offspring of the Sudra male and the Brahmini female to the outcast status of Chandala, may entail only economic convenience. The pratiloma caste distinction appears to be a sanction imposed to maintain the prestige of the Brahmin on which, according to N. K. Dutt, the whole caste system turns. It is designed to protect the Brahmin prestige from pollution, and freedom from pollution, as is emphasized in the chapter on caste strictures, is the very essence of the caste system. In this connexion it is most interesting to observe that certain very low castes can be polluted by Brahmins (pp. 162–3).

The jealousy of the Brahmin for the Kshatriya is hardly sufficiently stressed; the old literature is full of it. In the period of the Rig-Veda the Brahmin was the family priest and did not rank as high as the warrior. His importance as a professional ritualist in a system derived, as Prof. Hutton believes, from a consciousness of soul-stuff, mana, magic and taboo, increased rapidly as more and more of the original inhabitants, who had these beliefs, were included in the Hindu fold. It is increasingly more certain that Brahminism and Brahmins were pre-Vedic and that they emerged to their full importance after the period of the Rig-Veda when pre-Aryan influences became stronger. It is significant that the early caste Hindus from their stronghold of Madhyadesa looked on the Aryans of the Saptasindhu, the old Rig-Vedic region, as 'Nichiyas and Apachyas, low-born, ill-mannered Westerners' (Aitareya Brahmana, VIII, 14).

The ideas of soul-stuff, mana, magic and taboo played, in the opinion of Prof. Hutton, a leading part in the origin of caste: he says—'It is not suggested that the caste system developed from ideas of Soul-stuff, mana, magic and taboo alone; only that without these ideas it could not have developed'. Enough perhaps is not made of the doctrine of punya (merit) and pap (sin) as enlarged upon by Abbott in his *Keys of Power*, though J. Wilson's *Indian Caste* is quoted in this connection where he says of caste: 'It unfolds the ways of committing what it calls sins, accumulating sin and putting away sin, of acquiring merit, dispensing merit and losing merit'.

An origin in ritual for the caste system was put forward by Hocart in his book *Les Castes*, and it can easily be seen that belief in a punyavant who, to quote Abbott, by his store of merit can accomplish anything, the mere sight of whom uplifts and who can bring salvation to the meanest person, implies both ritual to acquire and renew this merit and taboo to safeguard it; such persons, filled with punya or mana, being of considerable value to the community. It is likely therefore that both Hutton and Hocart are right in regarding ritual, taboo and belief in mana as lying at the roots of the caste system.

Until relatively recently Hinduism was bounded by the Rig-Veda which in its earliest hymns contained little that can be associated with Brahminical Hinduism. Prof. Hutton gives due weight to the great influence of what now must be recognized as pre-Vedic Hinduism. He also introduces the extremely important factor of probable pre-Vedic Aryans, to which can be added pre-Aryan Iranians. At the same time it is unlikely, as the author points out, that the roots of the caste system lay in any of these peoples. It is essentially Indian, and it is to the Indus and possibly the Ganges that we have to look for the proto-Brahmins and the rise of the Hindu religion and the caste system. In this connexion terracottas found at Buxar are cited as suggesting an extension of the Indus cultures to the Ganges; actually these terracottas are Sunga and the location of their find spot needs to be received with considerable reserve.

As regards the future we are not told very much. The possible results of the almost inevitable clash with communism is not touched on and the ending of British rule in India, not yet a *fait accompli* when this book was written, which should not affect caste but probably will, is not considered. A study of Prof. Hutton's book will not only give one a great deal of information about caste but will correct a number of wrong impressions, for he shows that in spite of such regrettable features as untouchables—and even unseeables—there is much in caste that makes for social and racial adjustment in a huge area with a vast population in every stage of development from the neolithic upwards. D. H. GORDON.

SCOTLAND BEFORE THE SCOTS. *By* V. GORDON CHILDE. *Methuen*, 1946. *Octavo pp.* 96, 12 *appendices*, 16 *plates.* 12*s* 6*d.*

This work appeared in embryonic form in 1935 as the final chapter of *The Prehistory of Scotland*: it was expanded under the aegis of the Rhind Lectureship in 1944 as *The Development of Tribal Society in pre-Roman Times* and it attained maturity under its own name two years later. The author endeavours in a novel way to present a slice of Scottish social life by applying to prehistory the principles of Marxism as practised by Soviet scholars. He claims that the narratives which these writers have produced seem more historical than a mere succession of invasions, while they are just as objective, being solidly based on observed data.

From the archaeological evidence the writer distinguishes six stages, from the earliest definable groups of immigrants who arrived after the glaciers had receded, to the Iron Age when the sixth stage is reached and history emerges with the arrival of the Roman Legions under Agricola in A.D. 80. From a state of primitive communism society had become divided into classes, still illiterate and barbarian, one of which—a small group of war-chiefs and charioteers—having acquired land by force spent its resources on armaments and objects of parade rather than on reproductive works.

The author's time reckoning is somewhat confusing and, although carefully stated, it is not easy to cope with a chronology divided up simultaneously into stages not representing equal periods of time or synchronous as measured against an absolute time scale. And, when he inverts their chronological and stadial order by describing the Neolithic settlement at Skara Brae before the Megalithic society which preceded it, in order to emphasize a primitive communism in its Marxian sense—where flocks and herds are held in common, but personal possessions in the form of gear, clothing, and ornament were not precluded as private property—confusion is only deepened. The collective tombs of the Megalithic people are regarded as symbolic of a social unit, organized on a kinship basis with the means of production commonly owned. The chambered cairn was the communal burying place of a clan, and, ergo, the symbol of a clan society, and, therefore, communistic.

The invasion of the Beaker-folk stimulated the historical development and with them were released new productive forces. The increased output of the metal industry of the Bronze Age was diverted to the use of warriors. The most talented craftsmen in bronze found patrons in the courts of La Tène war-lords. The thers had to sell their meaner skill to the rustics of Scotland and thus they may have been fitted into the kinship system as slaves of the local clan chief. Thus, it is claimed, the exploitation of labour may have been introduced.

The subsequent brochs, and even the Gallic forts, are memorials of an acquisition of Scottish soil by invaders from overseas and it is argued that if land can be acquired by the same respectable method of collective robbery as acquired cattle and commodity wealth, land itself may have come to be regarded as a commodity and a possession which could be owned, even privately, and thus the last vestige of primitive communism would have vanished. One cannot escape the feeling that the author is on the defensive. 'It cannot', for example, 'be un-Marxian to assert that the first cultivated plants and domestic animals were brought to Scotland—not by the wind and waves—but by actual human cultivators and herdsmen who came and settled on our soil'. The whole thesis would have been immeasurably strengthened by a less insistent enunciation of political doctrine.

The evidence which Prof. Childe has so carefully collated would have carried even more conviction if his statistical data had been more firmly grounded. The Scottish chambered cairns number some 360 of which only 66 have been systematically excavated and of these only 35 have yielded significant relics. Again, in only some 50 graves (out of some 665) did any weapon or ornament accompany interments in cinerary urns, while the deductions from the domestic record are, in the main, derived from a mere half dozen of native sites. The statistician might well be non-plussed by the conclusions drawn from data so fragmentary.

The author has not been particularly happy with Scottish place-names and one is kept wondering where he picked up his spelling of Shewalton, Dunecht, Baillieston and others.

A few of his assertions might be usefully qualified. 'No flint dagger has been found in any Scottish grave'. Evans mentions one, 6¾ inches long and 1½ inches wide, found in a cairn at Guthrie, Forfarshire, while Professor Daniel Wilson records another, found in a cairn at Craigengelt, near Stirling. Again, to designate the Scottish earth-houses—the long, low, narrow, subterranean galleries—as funk-holes, comparable to our air-raid shelters, into which women and children could retreat in safety during a cattle raid when the men folk were away, appears to be straining unduly the slight evidence available. The locus of the earth-houses, like that of the brochs, is predominantly in the far North; very few are found in Southern Scotland. Yet raiding, either for domestic or warlike purposes, could hardly have been so geographically localized as the distribution of the earth-houses suggests.

But these trifles do not seriously detract from a very skilful piece of work. In a masterly manner Prof. Childe has recreated the dry bones of Scottish archaeology and has given, with the assistance of excellent photographs and drawings, a vivid and imaginative reconstruction of early social and domestic life in Scotland. While he has written with a red pencil and viewed his landscape through deep-rose coloured spectacles, he has compiled a useful compendium which will be of considerable value in interpreting the Scottish Prehistoric background and in assessing its place in the general European framework.

J. M. DAVIDSON.

Antiquity

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Editorial Notes

OF all the modes of travelling a journey on a river-boat is perhaps the most pleasant, combining comfort with an ever-changing scene. These notes are being written on the 'Kirbekan', an ancient steamer that plies on the Nile between Karima and Dongola—a distance of about 160 miles, between the 4th and 3rd cataracts. It is an almost rainless region with a winter and spring climate that would be perfect if it were not for the nimitti, a kind of biting midge that swarms in the Dongola reach at this time of year; and even the nimitti are less tiresome on a boat. There are frequent halts to embark passengers and cargo (chiefly sacks of dried dates), and opportunities for short excursions. The speed of the 'Kirbekan' is not great, and we have twice gained time for visiting ancient sites by disembarking and rejoining further on. The whole region is archaeologically almost unknown; the sites of some of the principal antiquities are known, but many of them have been seen perhaps once only, and that not for more than a century. There are no plans of any of the sites, and the only descriptions are those of travellers whose visits were (like the present writer's) too brief to be adequate. The best general account is still that of Lepsius, who passed through the region nearly a century ago.

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The remains belong to three periods: those of the Ethiopian kingdom (*circa* 750 B.C.–A.D. 350) whose capital was at Napata, now Merowé (not to be confused with the later Ethiopian capital Meroe much higher up the Nile near Shendi); those of the Christian Kingdom of Dongola (*circa* A.D. 550–1350); and those of the Fung period (*circa* 1504–1820). Between each was a Dark Age of some two centuries. The Napatan sites have been excavated by Dr Reisner, and publication is expected shortly. They are confined to the neighbourhood of Merowé and consist of pyramids and temples. The Christian sites are scattered throughout the whole region and consist of churches and forts. The Fung remains are of mud and stone castles.

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We disembarked at Khandaq where on a rocky bluff above the river are the remains of a gaunt castle of rough stone whose ramparts are reinforced by buttresses and walls of unburnt mud brick. The ground is thickly strewn with painted and other potsherds of the kind called ' Christian ', but not otherwise datable. In the lower parts of the tower are squared ashlar blocks of sandstone, probably reused from some earlier building. Round the castle is a small village. It was once an important place of call on the great caravan route from the south to Egypt, but like all the Dongola reach it is now by-passed and decaying. After examining the castle we took the bus that plies regularly between Omdurman and Dongola (300 miles, of which the first 200 are across the desert to Debba). Bus travel is always entertaining and the passengers were helpful in pointing out the sites of places named by travellers but not marked on the only existing map.

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This map deserves a word of description. It is on a scale of 1 : 250,000 (roughly 4 miles to 1 inch), covers the whole Anglo-Egyptian Sudan, and is the largest scale map available. Field-work with a map on so small a scale would be very difficult even if the map were up-to-date and accurate, but it is not. It is particularly inaccurate in its representation of the numerous Nile islands, which are constantly disappearing or appearing elsewhere. Consequently it is often difficult to locate one's position on it. A country that has no adequate maps must be difficult to administer; and one would have thought that a programme of air-survey might have had priority over some other forms of expenditure.

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The outstanding site is Old Dongola, now a wretched village only. Here was once a large town, for 800 years the capital of a Christian Kingdom whose northern frontier was at the 3rd cataract. Old Dongola is 70 miles above the modern town which has appropriated its name and which dates only from the Turkish conquest of 1820. As one approaches from the south one sees a great bare berg hill on whose flat top sits a small and featureless but massive square building. This is an ancient church still intact and used as a mosque. Our boat did not stop there, and in order to see it we had to disembark at Amentigo, cross the river to Rumi, ride in a car (which by good luck happened to be available) to Sellenarti and recross the river. The ferry at Amentigo was one of those flat-bottomed boats described by Hornell (see ANTIQUITY, 1943, XVII, pp. 27–41). The sail is wound and unwound by a simple but effective device. The car ride is best forgotten. The driver was ' full ' and the radiator empty. We bumped and roared over a succession of cultivation-banks and entered a howling wilderness of sand and mirage. Fortunately we did not stick and it was only about 12 miles to the river again. We hailed a ferry-boat which put back and picked us up. From where we landed on the right bank it was about a mile and a half to Old Dongola. We had to walk quickly as we had only a couple of hours before rejoining the ' Kirbekan ' at Ghaddar. The air was brilliantly clear and the famous old hill with its church stood out well. As we neared it we passed through a maze of low mounds covered with broken red bricks characteristic of Christian sites all over the Sudan. Here was the town, and the ground was covered everywhere by thousands of potsherds, many of them of the typical painted kind that seem to date before the middle of the 14th century, but may be much earlier also. At the foot of the hill are the granite columns of a church, the capitals, with faintly discernible carved designs, lying near by. Here too are the wind-eroded remains of post-Christian houses, their sun-dried bricks full of Christian potsherds.

We climbed the hill and were conducted into the building. The church, still in use as a mosque, is on the first floor, and below it are some vaults that contain a well, but there seems also to be much space unaccounted for. In the church are five granite columns (the sixth replaced by a wooden substitute), supporting the flat roof that has taken the place of the original dome. The walls are covered with plaster; but where parts of it have been removed on the west and north walls, the remains of rather weathered paintings can be seen—two heads on the west wall, but nothing definite elsewhere. The light is dim, and in a better light one might see more. Across the nave has been built a mud wall with a *mihrab* niche and on it is a dated inscription in Arabic stating that the building was converted to the use of Islam in A.D. 1317. (None of the books referring to this inscription give the actual text or a translation of it). There is a semicircular apse at the end of the church. The east wall is seen on the outside to be built of the typical baked red brick.

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The whole site calls aloud for a detailed survey on modern lines, with plans and photographs (including air-photographs). It is one of the most important in the Upper Nile region, and the church one of the oldest in Africa still extant and more or less intact. If the plaster were removed it seems certain that a whole mass of paintings would be revealed, and good hope, from what is now visible, that they may have been left undamaged. The removal of the plaster, however, is obviously not to be undertaken without adequate safeguards for the future.

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Our boat had the excellent habit of tying up at night, so that one did not miss much of the scenery. Two days later we passed Korti and entered the Shaigia reach where the Nile flows south-westwards for over a hundred miles. The Shaigia are Arabs, and the only people who made any attempt at resisting the Turks in 1820. The decisive battle (which they lost) was fought at Jebel Dega, now called Jebel Ibn Auf. From here onwards the right bank is fringed with palms, through which one can just discern the low sandstone cliffs, and catch a glimpse of the granite columns of a ruined church. We reached Maqāl and tied up at sunset, but there was just time to visit a fine fortress on the low hill there, immediately above the boat station. It is rectangular with 18 semicircular towers round the walls, which are 16 feet thick, or more in places, and consist of a thick wall of dry stone on each side, enclosing a core of red and burnt brick. Across the spur on the N.W. (or land) side are two deep trenches, the outer one at least 12 feet deep, resting on small khors on each side. The castle thus resembles a promontory fort. Its age is indicated by the red brick core and quantities of Christian pottery. Forts of similar construction have been recorded at Kubinat and El Kab above the 4th cataract. Nothing is known about their builders.

❧ ❧

At Merowé, in the old village on the right bank, are the remains of four Shaigia mud and stone castles. These are much later and of an entirely different type Essentially they are enlarged and strengthened versions of the courtyard house which is still today normal in the Nile valley between Berber and Shendi. It consists of a rectangular mud wall incorporating several single-roomed buildings. In the castles these have more than one floor and take on the semblance of towers. But there is no symmetrical disposition of the towers, as in the earlier forts. The courtyard house is, in plan, simply a mud, or mud and stone, version of the primitive African zareeba dwelling or kraal, with the conical straw huts transformed into flat-roofed mud buildings, and the enclosing thorn zareeba replaced by a mud wall. Examples of the courtyard type of house are to be seen from the train window between Damer and Shendi as these words are being written. At the Turkish conquest in 1820 these Shaigia mud castles were still inhabited by the tribal chieftains, and full descriptions of them are given by the travellers who accompanied or followed behind the Turkish army—Cailliaud, Waddington, English and Hoskins. The largest at Merowé was taken over and used by the Turks, and the fact is still remembered locally and recounted on the spot. No plans of any of these castles have ever been made, and it is most desirable that this should be done before they have fallen into complete decay.

⁂ ⁂

Our river-trek was one of the most enjoyable archaeological excursions that can be imagined. The scenery is varied and in places very beautiful. The early mornings, before the flies began to torment us and while it was still cool, were delightful. In this dry region the air has a clarity that gives an added beauty to the landscape—the bright yellow sand on the right bank with its brilliant green trees, the morning star casting its reflection on the calm water, and the purple hills beyond. The river is everywhere very wide, sometimes as much as half a mile, and the air therefore cool, even at midday. Even the minor discomforts are offset by the joy of making an occasional new discovery.

⁂ ⁂

These rather disorderly notes cannot close without some expression of gratitude for the kindness and hospitability of the people we met during our travels. More than once we were pleasantly surprised to meet a reader of ANTIQUITY, even in the remotest place we visited ; and it was most gratifying to find so much real interest in the archaeology of the Sudan. On the return journey the writer was entrusted with two newly found Meroitic pots which were brought to the train to be conveyed to Khartoum Museum. Thus concluded a most successful tour.

⁂ ⁂

A postscript written in the flying-boat over Sicily about 25 miles west or northwest of Catania. On a sloping mountainside still cultivated the unmistakable rectangular pattern of Roman centuriation is plainly visible. The upper limit is a broad belt running for several miles along the slope some distance below the top of the ridge and parallel with it. The ancient lay-out is recognizable by straight lines, broken in places, but plainly once continuous. The system resembles that near Split described by Mr John Bradford (ANTIQUITY, 1947, XXI, pp. 197–204), but it is rather less striking. This is the first time centuriation has been observed in Sicily.

The Classical Rhinoceros

by SIR WILLIAM GOWERS

BY the term ' classical rhinoceros ' I mean the rhinoceros which was known to the Greek and Roman world during the five and a half centuries between 300 B.C. and A.D. 250, which was shown from time to time at Alexandria under the Ptolemies and later on appeared regularly in the arena at Rome taking part in fights with other beasts and with men. Although the Indian rhinoceros seems occasionally to have been exhibited at Rome, at any rate in the early years of the Empire, I believe that the rhinoceros usually shown there came from Africa, and I have tried to analyse such evidence as is available to show firstly what species it was and secondly what part of Africa it came from.

There are, of course, two quite distinct kinds of African rhinoceros, the square-mouthed and the prehensile-lipped, popularly known respectively as the White and the Black Rhinoceros. Until recently their scientific names were *Rhinoceros simus* and *Rhinoceros bicornis*, but systematists have now separated them into two genera, calling the former *Ceratotherium simum* and the latter *Diceros bicornis* ; denying to both the title of *Rhinoceros* which they reserve for the Indian rhinoceros and its near Asiatic relatives. For the sake of simplicity and brevity I shall retain the old names and call them *simus* and *bicornis*.

The popular misnomers of ' white ' and ' black ' are a legacy from the South African Dutch of the 17th century, who called *simus* ' wit renaster ' and *bicornis* ' zwart renaster'. They were not very particular about exact shades of colour and probably meant no more than that one species usually appeared much lighter than the other. The natural colour of both appears much the same to an observer a little distance away. The hide of *simus* may be slightly lighter. Perhaps the most accurate definition is given by Roosevelt and Heller (1) who say that the true colour of *simus* is smoke-grey while that of *bicornis* is dark clove-brown.

The apparently light colour of *simus* which impressed ancient as well as modern observers is as a rule due to the habit, to which it is much more addicted than *bicornis*, of wallowing daily in muddy pools, the mud from which drying on its hide gives it often a much lighter colour than its natural one, the precise shade depending on the nature of the soil. Cornwallis Harris (2) who more than a century ago observed large numbers in South Africa—as many as eighty in one day's march—writes ' The outer coat acquired by constantly wallowing in pools often approaches cream colour '. *Bicornis* generally shows up in its natural dark colour but never looks really black like, for instance, the Cape Buffalo.

Simus is purely a grazing animal and its food consists of nothing but grass and such weeds as may accidentally be taken up with grass as cattle may do. *Bicornis* is a browser living almost entirely on the leaves and shoots of bushes, occasionally eating roots. *Simus* is much more dependent on water, both for drinking and wallowing, than is *bicornis* which can exist in arid and stony localities where *simus* could not survive. Finally,

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[1] Theodore Roosevelt and E. Heller, *Game Animals of Africa* (1915), p. 670, see also Herbert Lang, ' The White Rhinoceros of the Belgian Congo ', *N.Y. Zool. Bulletin*, July 1920, p. 75.

[2] Capt. (later Sir W.) Cornwallis Harris, *Portraits of Game and Wild Animals* (1840), p. 98.

simus is much the larger animal, being indeed next to the elephant the largest land mammal, and probably weighs at least half as much again as *bicornis*.

Since the evidence I am concerned with is largely pictorial I will mention some characteristics of *simus* some of which are always shown (sometimes emphasized or exaggerated) in ancient representations of it. They are (1) the great size and length of the head in proportion to the body, (2) the low downward-pointing carriage of the head; when travelling or standing at ease the muzzle nearly touches the ground, whereas the head of *bicornis* is held nearly horizontal, (3) a pronounced nuchal hump, just in front of the shoulders, (4) a great disparity in size between the front and rear horn—much greater than is usually found in *bicornis*, (5) a habit peculiar to *simus* when alarmed or excited of carrying its tail curled in a circle or ringlet (FIG. 7). *Bicornis* never does this; its alarm signal is to raise the tail vertically as shown in FIG. 8. A curled-up tail is a certain indication that the animal depicted is *simus*.

The classical rhinoceros during the three centuries in which it appeared in Rome seems always to have been obtained through Egypt. The distribution of the rhinoceros in early times in the Nile basin therefore becomes relevant.

The earliest evidence about this is a rock drawing on a sandstone cliff to the west of the Nile at El-Hosh (Lat. 24–30 N) between Idfu and Assuan—discovered by H. A. Winkler (3), who states that it is the only rock drawing of a rhinoceros yet found in Egypt. It is shown in a procession of animals including elephant, antelope and gazelle. Winkler does not attempt to date it otherwise than by ascribing it to the ' autochthonous mountain-dwellers '. It looks rather like a caricature and not nearly so naturalistic as many of the earlier prehistoric rock carvings and drawings, e.g. that shown in FIG. 1 from much the same latitude in Libya (4). But it undoubtedly represents *simus*.

The next evidence chronologically is from the pottery models of rhinoceros horns found by Col. W. B. Emery (5) in the tomb of Hor-Aha at Saqqara—date about 3000 B.C. From the photographs they look very accurate models and their length and shape approximate to *simus* rather than *bicornis*. Moreover there is so far as I know no record of the existence at any time of *bicornis* in any locality whence the originals could have been obtained and brought to Lower Egypt at this date, whereas there is evidence of the existence of *simus* in Upper Egypt and Nubia both before and after the date of the Saqqara models.

The next item of evidence comes from Kerma about 30 miles north of Dongola (6), where there was an Egyptian trading outpost in Nubia and its date is between 1780 and 1680 B.C. It consists of an ivory or bone inlay and a raw-hide silhouette, both representing rhinoceros. The latter has its head damaged and the front horn broken off but I think it is certain, as already suggested by Hilzheimer, that both this and the inlay (FIG. 4) represent *simus*.

The latest evidence from ancient Egypt is provided by Mond and Myers, from Armant near Luxor (7). A frieze on a pylon of the temple here shows an animal said to be a rhinoceros; it must have been drawn by an artist who had never seen one alive

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[3] H. A. Winkler, *Rock Drawings of Southern Upper Egypt* (1938), vol. I, p. 21 and pl. XX and XXI.

[4] Paolo Graziosi, *L'Arte Rupestre della Libya* (1941) vol. II, pl. 134a.

[5] W. B. Emery, *The Tomb of Hor-Aha* (Saqqara Excavations 1939), p. 72.

[6] G. A. Reisner, *Excavations at Kerma* (Harvard African Studies, 1923), vol. VI, pl. 53a. M. Hilzheimer, *Zeitschr. f. Aegyptische Sprache*, vol. 69 (1931), p. 72.

[7] Sir R. Mond and O. Myers, *The Temple of Armant* (1940), p. 25, pl. 8 and 103.

and in itself is of no value as an indication of species. But here was also found a stele of Tuthmosis III recording *inter alia* that in the course of an expedition into Nubia he killed a rhinoceros. The pictograph is like *simus*, and since the Kerma images provide evidence of the presence of *simus* in Nubia two or three centuries earlier we may reasonably assume that Tuthmosis' rhinoceros was also *simus* and that it existed there in the middle of the second millennium B.C.

I know of no more evidence from ancient Egypt. In the Sudan, however, a rock-drawing of a rhinoceros, which must be *simus* has been recorded from Northern Darfur by A. J. Arkell, *Sudan Notes and Records*, vol. xx, p. 281. To come down to much more recent times we have the story in Pseudo-Callisthenes (8) of the eighty rhinoceros offered to Alexander the Great in 331 B.C. by a much antedated Candace queen of Meroe, as to which we may say that the legend would probably not have originated unless rhinoceros had been common in the Meroitic kingdom at that date.

There can I think be no doubt of this. We have the record of Ptolemy II's 'Ethiopian' rhinoceros (*infra*), and some 350 years later Pliny (N.H. VI, 35) referring to the exploring expedition sent by Nero to the Sudan in A.D. 65 says that they reported that when they reached the neighbourhood of Meroe (about Lat. 17 N.) *herbas demum viridiores sylvarumque aliquid apparuisse, et rhinocerotum elephantorumque vestigia.*

Assuming that these rhinoceros were of the same species as those recorded from Nubia by the ancient Egyptians—and there is no reason to doubt it—we seem to have a picture in the Nile basin of a gradual retreat by *simus* due to increasing aridity from a northern front of at least 25 N in the 4th millennium B.C. to about 19 N in the second millennium and 17 by the beginning of the Christian era (9). About this point in the Nile valley it was probably stabilized for a considerable time having reached a region where rainfall was sufficient for the grass it needed. The most northerly point at which *simus* has been recorded recently (1927) is Goz-Beida in Wadai in Lat. 12 N about 70 miles from the western frontier of Darfur (10).

An unfordable river is a complete barrier to the movement of rhinoceros and I feel doubtful whether the distribution of *simus* in Egypt and the Sudan ever extended to the east of the Nile. That it existed in Eastern Africa South of the Equator in prehistoric times is shown by the rock drawings from Kisese in Tanganyika figured by Leakey (FIG. 2) (11), but whether it existed throughout East Africa at this time and if so what caused its complete disappearance from territories where *bicornis* continues to flourish is a mystery as yet unsolved.

What we do know is that the migration of *simus* in modern times in an east south easterly direction from Lake Chad, Kanem, Bagirmi, Wadai and the Ubangi-Shari has been held up at the Nile on a front of some 400 miles north of Lake Albert. It seems to me possible that it only reached the Nile in these latitudes some 50 or 60 years ago. Otherwise its existence must surely have been detected by such observers as Baker, Junker Schweinfurth and Emin, who spent years in or close to the region where it is now

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[8] Pseudo-Callisthenes, *Hist. Alex Magni* III, 18 (ed. W. Kroll, Berlin, 1926).

[9] S. A. Huzzayin, *The Place of Egypt in Prehistory* (1941), p. 285, thinks there was in Saharan latitudes, in the 1st millennium B.C. until early A.D. a phase of increased rainfall which may be equated with Leakey's Nakuran wet phase. This, if so, would slow up the southerly retreat of *simus*.

[10] L. Lavauden, *Faune des Colonies Françaises* (1933–4), p. 420. Lavauden also records that in 1927 there were stored in Khartum, in transit from Wadai, no less than 150 *simus* horns. Major W. R. Barker, lately Game Warden in the Sudan, corroborates.

[11] L. S. B. Leakey, *Stone Age Africa* (1936), ch. VIII, fig. 27.

most common. The real discoverer or re-discoverer of the northern geographical—no doubt the original— race of *simus* was von Heuglin (**12**), but he met with it 200 miles or more West of the Nile.

The first African rhinoceros to be mentioned by classical writers was the one which was exhibited at Alexandria in the great procession in 284 B.C. at the festival of Dionysus which coincided with the accession of Ptolemy Philadelphus. It is mentioned by Athenaeus (v. 201(c)) on the authority of Callixeinus. It is described as 'Αἰθιοπικός, a term which often has a very vague geographical connotation but which at that date in Egypt must have meant that it came from Nilotic Ethiopia, i.e. the kingdom of Meroe, with the ruler of which Ptolemy was on good terms and from whom he may well have obtained it.

The only picture of a rhinoceros in Ptolemaic times is in a frieze in one of the rock tombs at Merissa (**13**) in Southern Palestine—the old Idumaea. It was drawn about 200 B.C., when Palestine was under Egyptian rule. The animal, labelled ' rhinoceros ', is among other animals associated with Egypt, which are well executed—next to it is an African elephant. But the so-called rhinoceros is evidently done from a description only and the only inference to be drawn from it is that it had been described to the artist as being like a gigantic pig, with a horn at the end of its nose and an extremely minute horn behind this.

The earliest description of an African rhinoceros that we have is from the geographer Agatharchides who wrote about 130 B.C. (**14**). He lived for a long time at Alexandria and, as he says, had access to the reports and memoranda, in the royal archives, of the early explorers of the Red Sea coasts and of the officers who were stationed there. The description comes in his account of the fauna of the ' Trogodyte ' country, i.e. the modern Eritrea and northern Somaliland. He says that the length of the rhinoceros is about the same as that of an elephant but its height is less. This would be true of *simus*, which may be as much as 13 feet long—about the same as the species of elephant known to Egypt at that time. Its colour, he says, is like boxwood ; here he agrees with the Dutch who called it white and with Harris's ' cream colour ' (*supra*). It has a bent-back (σιμόν) horn on the end of its nose as tough as iron. No mention is made of a second horn and this points to *simus* rather than *bicornis*. The rear horn of *simus* is often negligible, consisting of a mere knob so that an observer might well describe it as one-horned. Harris writes that while the front horn is a formidable weapon 3½ feet long ' the other horn is generally such a mere excrescence that at a distance of even a few yards it is barely perceptible ' (**15**). In the middle of the 19th century the Arab traders who brought *simus*

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[12] Th. von Heuglin, *Reise in das Gebiet des Weissen Nil* (1869), p. 361. *Simus* could not have travelled due south from E. Kordofan ; the Bahr-al-Ghazal would have prevented it.

In 1825 a pair of *simus* horns had been brought to England by Major Denham from the vicinity of Chad, but they were not then recognised as *simus*, which was believed to exist only in South Africa, where it had been ' discovered ' and named by Burchell some 10 years earlier.

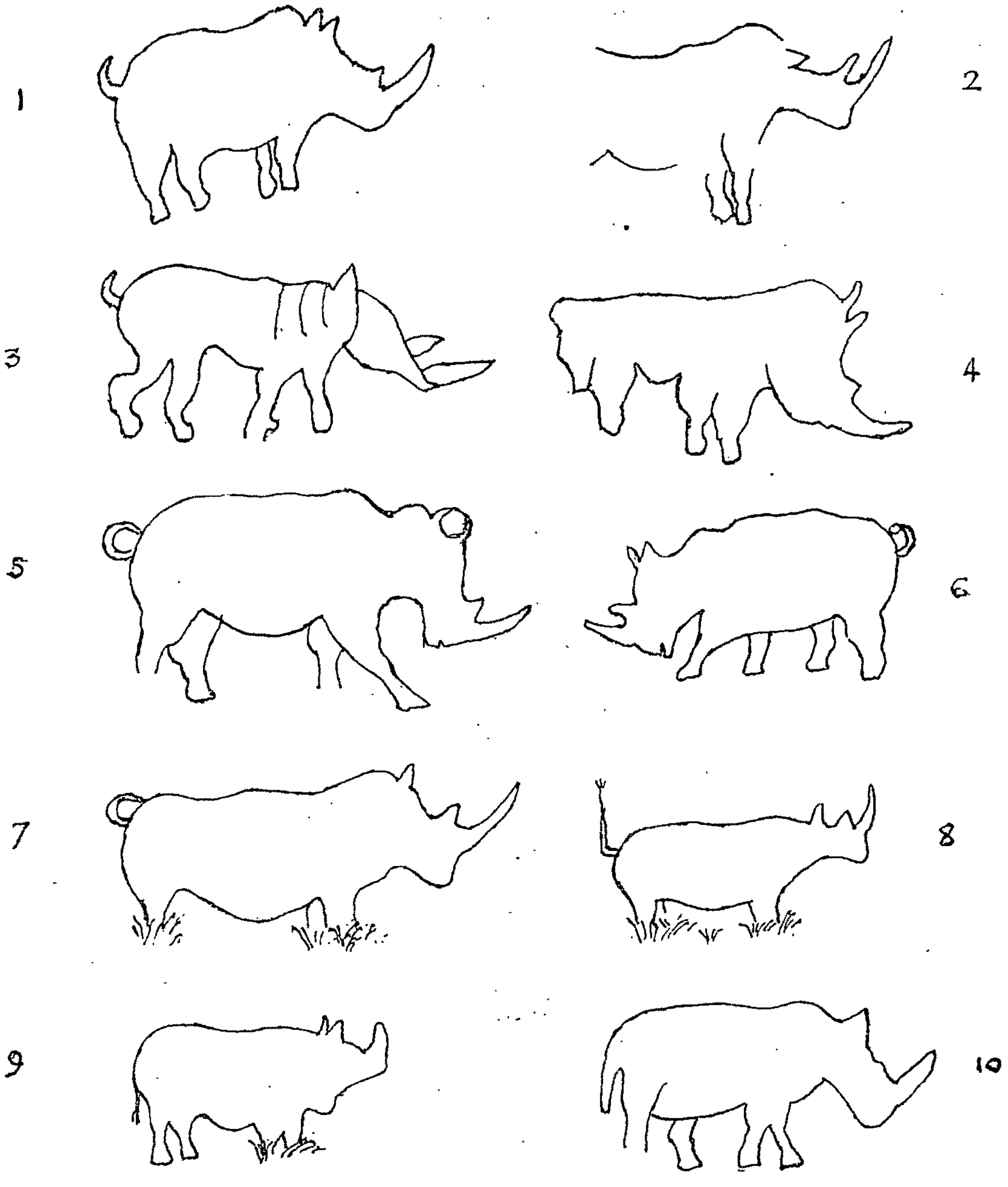
[13] J. P. Peters and H. Thiersch, *The Painted Tombs of Merissa* (1903), (Palestine Exploration Fund).

[14] Agatharchides, *De Mari Erythraeo* (P. 71), in C. F. Mueller's *Geographi Graeci Minores* (1856), vol. I.

[15] Op. cit., p. 98. Modern records show that the front horn of *simus* is on the average nearly four times as long as the rear horn while in *bicornis* it is a little less than twice as long. I am inclined to think that since *simus* has been confined to semi-forest country it does not develop such long front horns as it did when it lived on the open plains.

THE CLASSICAL RHINOCEROS

PROFILES OF RHINOCEROS (not to scale)



1. LIBYA PREHISTORIC
2. TANGANYIKA do.
3. UPPER EGYPT do.
4. NUBIA 1730 B.C.
5. ALEXANDRIA A.D. 91 (Coin of Domitian : × 3)
6. ROME A.D. 95 (Coin of Domitian : × 3)
7. R. SIMUS
8. R. BICORNIS
9. R. UNICORNIS

From recent photographs from life (7–9)

10. ROMAN TESSERA, 1st century A.D. (Rostovtsev No. 486) (× 3)

horns from Kanem and Wadai for sale in Tripoli and Jeddah were firmly persuaded that the animal had only one horn (16).

Agatharchides goes on to say that when the rhinoceros comes across a rock he sharpens his horn on it so as to be ready to encounter the elephant with which he constantly has disputes about the pasture – περὶ τῆς νομῆς – and which he often kills by driving his horn from underneath into the elephant's belly. I should not like to say that such fights were impossible where elephants and *simus* were in numbers on the same grass lands. I have seen an elephant drive off a buffalo that he thought was grazing too close to him, and *simus* might resent this. Roosevelt and Heller write 'One of the few white rhinoceros in the South African Game Reserve, a bull, was charged and killed by a stab behind the shoulder by a bull elephant which was also in the Reserve'. The belief in fights between the two was very persistent in ancient times; it is repeated by Diodorus, Pliny, Dio Cassius and Aelian.

One cannot believe the story of the rhinoceros deliberately sharpening his horn, but it is evidently an attempt to account for an actual characteristic of *simus*. The anterior horn always has its front surface flattened, looking rather as if it had been filed. Attention was first drawn to this by Selous in 1881 (17): 'I never remember to have seen a horn of the square-mouthed rhinoceros that was perfectly round; they always have the front surface partially flattened by friction and may thus at a glance be distinguished from the invariably rounded horns of the prehensile-lipped rhinoceros'. The flattening is caused by continual abrasion on the matted roots of grasses, perhaps also on the ground, as *simus* grazes with its nostrils nearly touching the ground and its horn held horizontally in front. By repeating the horn-sharpening fable Agatharchides actually gives a pointer to the kind of rhinoceros he is describing.

This rhinoceros may have lived on the Eritrean plateau or the coastal plain below it, where as we know from the author of the *Periplus Maris Erythraei* rhinoceros were still very common towards the end of the first century A.D.; and Pliny writes (N.H. VI. 173) that rhinoceros horns were an important item of export from the Eritrean port of Adulis. We have however no conclusive evidence that *simus* ever existed in that locality. Cosmas Indicopleustes (18) in the 6th century describes the rhinoceros he saw in this region in terms which seems to me to indicate *bicornis* rather than *simus*. *Bicornis* was widespread in Abyssinia and Somaliland 100 years ago and still exists in small numbers. I think it possible that Agatharchides took from his early authorities merely the fact that rhinoceros existed in the 'Trogodyte' country and, assuming that all rhinoceros were alike, relied very naturally for his description of the animal and his account of its habits on what he had seen and heard of the Nilotic rhinoceros during the years that he lived in Alexandria—as Artemidorus did after him. That he was describing *simus* I feel sure, but I do not feel so sure that it lived in the Red Sea territories as well as on the Nile.

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[16] E. L. Trouessart, 'Le Rhinoceros blanc du Soudan', *Proc. Zool. Soc. Lond.* (1909), p. 198; quoting Fresnel (*C. R. del'Ac. des Sciences Paris*, 1848), t XXVI, p. 281.

[17] F. C. Selous, 'The South African Rhinoceroses', *P.Z.S.*, 1881, p. 725.

[18] Cosmas Indicopleustes, *Topographia Christiana*, XI, 441, edit. E. O. Winstedt (1909). He says he watched a rhino from a safe distance and made a careful drawing from a specimen (stuffed) in the King's palace at Axum. This must have been lost as the picture in the 13th century 'Laurentian' MS cannot have been drawn by Cosmas or anyone who had seen a rhino. His description gives it two horns and a hide rather like an elephant. It is φοβερώτατον πάνυ μάλιστα. (Africans generally are terrified of *bicornis* but not afraid of *simus*.) It is possible that both species co-existed in Eritrea in Ptolemaic times and that by Cosmas' day *simus* had been exterminated there.

The next description is by Artemidorus, who wrote some 30 years after Agatharchides, in a work on the Red Sea which Strabo (XVI, 4, 15) quotes in giving an account of the fauna of the Somali coast between the straits of Bab el Mandeb and Guardafui. His description closely followed that of Agatharchides (whom Artemidorus generally follows) and to have been confirmed by his own observation of a rhinoceros, no doubt *simus*, which he had seen at Alexandria, probably in the royal Zoo. But Strabo, writing a century after Artemidorus disagrees with the description. He disagrees as to the length of the body and as to the colour, which he says is not like boxwood but more like that of an elephant. Its height he says is that of a bull and in shape it is most like a wild boar expecially about the head except for the horn on its nose 'tougher than any bone'. He goes on: *ἔχει δὲ τύλους δύο ὡς οὖν σπείρας δρακόντων*—it has two raised ridges like the coils of snakes, from the spine to the belly, one at the shoulders one at the loins. Strabo is certainly describing the folds of skin of an Indian rhinoceros which he had probably seen at Rome; if he had seen it at Alexandria, where Artemidorus saw his, I think he would have said so; and there is no reason to think that Indian rhinoceros were ever brought to Alexandria. To him and his contemporaries a rhinoceros was just a rhinoceros; he had no idea that a rhinoceros from India was a quite different animal. He had to believe the evidence of his own eyes and not unnaturally concluded that his usual authority Artemidorus had made for once an unaccountable mistake. Suetonius (Div. Aug. 43, 4) says that Augustus, apart from the regular *spectacula*, used to exhibit specially any noteworthy rarity which might have been brought to Rome, and showed thus a rhinoceros in the Campus Martius and a tiger on the stage. Pliny (N.H. VIII, XXIV, 65) tells us that in 11 B.C. Augustus exhibited a tame tiger—the first tiger ever seen at Rome; and it may be that the rhinoceros which Strabo saw was the one mentioned by Suetonius as being shown at the same time as a tiger. Both were probably gifts from Indian rulers who from time to time sent embassies to Rome with offerings which on one occasion included elephants (Florus II, 34).

A marble relief from Pompeii figured by Keller (**19**) shows a rather conventionalized ndian rhinoceros dressed in some fabric cut so as to resemble a coat of armour; the design may have been based on this or another rhinoceros similarly exhibited. I know of nothing to indicate that at Rome an Indian rhinoceros was anything but a great rarity.

The rhinoceros was first seen at Rome according to Pliny (N.H. VIII, XXIX, 71) in 55 B.C., at the great *spectacula* staged by Pompey in the year of his consulship. Pliny describes it as *unius in nare cornus, qualis saepe visus*. This has led Warmington (**20**), followed by Jennison (**21**), to suppose that from this date to the time of Vespasian it was he Indian rhinoceros (*Rhinoceros unicornis*) which was regularly seen in the arena at Rome. But Pliny was merely copying Agatharchides, as the rest of the passage shows, and I cannot believe that Agatharchides was describing, in 130 B.C., an Indian rhinoceros. Solinus (43) while following Pliny's description implies clearly that Pompey's rhinoceros came from Ethiopia. The spectacles of 55 B.C. coincided with the date of the restoration o the throne of Egypt (largely by Pompey's help) of Ptolemy Auletes; and it is quite natural to suppose that Ptolemy supplied an Ethiopian rhinoceros for the festival. Jennison indeed suggests that Ptolemy supplied the rhinoceros but that it had been imported to Egypt from India, which seems to me highly improbable.

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[19] Otto Keller, *Die Antike Tierwelt* (1909), p. 387.

[20] E. H. Warmington, *Commerce Between the Roman Empire and India* (1928), p. 151 and note p. 360.

[21] G. Jennison, *Animals for Show and Pleasure in Ancient Rome* (1937), p. 34.

There is a life-like representation of a White Rhinoceros in a black and white mosaic in Perugia (figured in *Notizie degli Scave* (1877) pl. XI) attributed to the time of Augustus. The rhinoceros appears among other animals being charmed by Orpheus; they include elephant, aurochs, stag, lion, leopard and bear. All of these could no doubt be seen at that time either in the arena or in the *vivaria* on the outskirts of Rome; they give the impression of having been drawn from life. The rhinoceros is shown as practically one-horned, with an indication of a rudimentary second horn just behind the first. If Pliny had seen such an animal he would certainly have described it as *unius cornus*.

Figures of rhinoceros appear on some of the leaden *tesserae* (22)—tokens or counters—which under the Roman Empire were issued to the public for the purpose of giving admission to the *spectacula*. Some of these show only one horn, and Warmington (op. cit.) regards them as representing Indian rhinoceros. But the Indian rhinoceros has a comparatively small head, held rather high (FIG. 9); very different from the massive down-pointing head shown on the *tesserae* (FIG. 10). This I regard as a more reliable indication of species as between the Indian *unicornis* and the African *simus* than the presence or absence of a second horn in these rather primitive designs. On all the *tesserae* I have seen I believe the rhinoceros figured is *simus*. One, in the British Museum, has a rhinoceros on one side and a crocodile on the other, suggesting a Nilotic origin for both.

A rhinoceros is shown in a section of the mosaic now in the Palazzo Barberini at Palestrina (Praeneste) (23) which purports to depict the scenery of the Egyptian Sudan including the Nile and its fauna. There are two small horns, very close together, and a pronounced hump behind the head. The mosaic probably dates from the 2nd century.

The next rhinoceros, after Pompey's, mentioned by the ancient writers was seen in 29 B.C. at the shows put on by Octavian to celebrate the defeat of Antony and Cleopatra and the annexation of Egypt. Dio Cassius (LI, 22, 5) records that among the beasts slaughtered were a rhinoceros and a hippopotamus, both seen then for the first time at Rome. (*πρῶτον τότε ἐν τῆ Ῥώμῃ ὀφθέντα*). In saying this Dio ignores Pompey's earlier rhinoceros, and also the hippopotamus shown by Scaurus in 58 B.C. The occasion of the spectacles and the juxtaposition of rhinoceros and hippopotamus strongly suggest that the former came from Egypt, as the latter must have done.

Dio records that towards the end of the reign of Augustus, in A.D. 8, an elephant defeated a rhinoceros in a fight in the arena, but nothing is said as to its origin.

After this there is a long gap in the literary records about the rhinoceros but we need not infer that it did not continue to be exhibited regularly. Pliny's *saepe visus* and its images on the *tesserae* suggest that it was.

The next literary allusions come from Martial's *De Spectaculis*—a book of epigrams written to commemorate the opening of the Colosseum by the Emperor Titus in A.D. 80 Epigram 22 is well known—

> Sollicitant pavidi dum rhinocerota magistri
>   seque diu magnae colligit ira ferae .
> desperabantur promissi praelia Martis
>   sed tandem rediit cognitus ante furor.
> Namque gravem cornu gemino sic extulit ursum
>   iactat ut impositas taurus ad astra pilas . . .

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[22] M. I. Rostovtsev, *Sylloge Tesserarum Plumbearum* (St. Petersburg, 1903). A good example of a rhinoceros with one horn which is clearly *simus* is no. 486, tab. IV, 7.

[23] G. E. Rizzo, *Pittura Ellenistica Romana* (1929), pl. 188, 189.

and Martial goes on to tell how the rhinoceros defeated a *bubalus* (probably an aurochs) and a bison, while a lion fled headlong from it.

The difficulty of the keepers in rousing it to the requisite pitch of irritation suggests the placid *simus* rather than the testy *bicornis* ; but one has no need to speculate about its species since conclusive evidence is supplied by two bronze coins of Domitian, whose reign began the year after the *De Spectaculis* was published. Suetonius says of Domitian '*spectacula assidue magnifica et sumptuosa edidit*' and he may have taken a special interest in rhinoceros.

The earlier of these coins (24) was struck in Alexandria, for circulation in Egypt, in A.D. 91, the second in Rome four years later. (25) . A rhinoceros occupies the whole of the reverse of each. Outlines of the animals taken from enlarged photographs are shown here as FIGS. 5 and 6. No one familiar with the African rhinoceros can I think feel any doubt as to the species they represent. The nuchal hump is not well shown but the curled tail, the square mouth and the size and carriage of the head are characteristic of *simus*.

A coin of Hadrian (26) (mint of Alexandria) of A.D. 127 also shows *simus* but the design is somewhat defaced and not so clear as in the coins of Domitian, though the large size of the head is even better indicated.

Antoninus Pius (*Hist. Augusta*, A.P. X) is said to have shown on one occasion (perhaps A.D. 149) elephants, hyenas and tigers—which probably came from Asia—and rhinoceros, crocodiles and hippopotami (from Africa). The historians and biographers make no allusion to it during the reign of Marcus Aurelius (as one might expect) but Commodus is said by Dio to have killed more than one rhinoceros in the arena and Caracalla also killed one. The *Historia Augusta* (El. XXVIII, 8) speaking of the collection of animals which Elagabalus (A.D. 220) kept at Rome says *habuit et hippopotamos et crocodilum et rhinocerotem et omnia Aegyptia quae per naturam sui exhiberi poterant*, which indicates that the rhinoceros was still considered an essentially Egyptian product.

Pausanias (*c.* A.D. 170) writes—more accurately than his contemporaries—that he saw at Rome 'Ethiopian bulls' called rhinoceros because they have a horn on the end of the nose and another smaller one behind it. Aelian (*c.* A.D. 215) in his *Historia Animalium* writes that it would be merely tedious for him to describe its appearance since so many Greeks and Romans had seen it—and then proceeds to repeat Agatharchides almost word for word (27).

The latest mention of the classical rhinoceros at Rome is in the *Historia Augusta* (Gordiani Tres. 33). Among the animals collected for Gordian's Persian triumph, and eventually shown by Philip the Arabian in A.D. 248, were six hippopotami and one rhinoceros. That this should be the last mention of the rhinoceros at Rome is just what we should expect if, as I believe, it was always obtained from Ethiopia. For in A.D 250 occurred the revolt of the Blemmyes and their attacks on Egypt, which inaugurated a period in which Rome lost for ever all control and influence in Nubia and the Meroitic kingdom, which itself was conquered by the Axumite Abyssinians a century later.

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[24] B. M. C. *Domitian* no. 333. An interesting feature of this coin is the sphere or disk on top of the head between the ears.

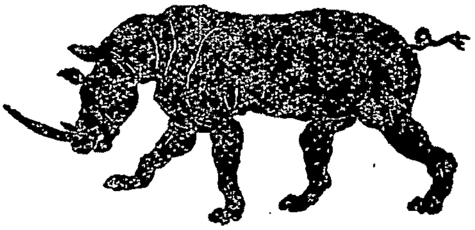
[25] B.M.C. Cat. *Roman Empire*, vol. II, p. 411 and pl. 31, 17.

[26] B.M.C. *Hadrian*, no. 835.

[27] Agatharchides' original description of the African rhinoceros, copied by Pliny, who very likely had never seen one, became the standard description and was used by e.g. Dio Cassius and Aelian long after the second horn had been shown on coins.

After A.D. 250 it would, I think, have been impossible for the Romans to obtain any more rhinoceros from the Sudan.

There does not seem to have been any other source from which the Romans could have regularly obtained rhinoceros during the three centuries during which they were shown at Rome. In prehistoric ages *simus* was widespread throughout North Africa and the Sahara and it is just possible that it may have lingered in Roman Africa even into historical times in a few climatically favoured spots. But no writer refers to its existence there and even the Saharan rhinoceros is, I think, only once referred to—in the well known passage of Ptolemy (I, 8) quoting Marinus of Tyre's description of an expedition by Julius Maternus at the end of the 1st century A.D. which started from Leptis Magna and passed through the country of the Garamantes (Fezzan) with the object of subduing some rebellious ' Aethiopians ' in the South. At its furthest south the expedition reached the region of Agisymba ' where the rhinoceros congregate '. It seems most likely that Agisymba was the Tibesti plateau (about Lat. 20 N) (**28**). The wording does not suggest that there was anything remarkable in seeing rhinoceros on the journey, only in seeing so many in one locality. The explanation no doubt is that as the desiccation of the Sahara and the Libyan desert progressed the higher plateaux such as Tibesti were the only localities which still had enough rainfall to provide water and grazing for *simus*—at any rate in any numbers.



RHINOCEROS ON ROMAN MOSAIC, PERUGIA

The province of Roman Africa being excluded as a source of supply, the only remaining alternative to the Sudan is the coastal belt of the Red Sea—Eritrea and Somaliland. This would have involved shipping the rhinoceros to Myos Hormos (Kosseir) or Berenice, with an overland journey to the Nile (**29**). The Ptolemies did this in the 3rd century B.C. with elephants, which were all-important to them, but it seems very unlikely that either they or the Romans ever did it with rhinoceros. There was no need to; transport down the Nile from the Meroitic kingdom would be simpler and easier. Moreover during the period in which *simus* was regularly brought to Rome it is not certain that it existed in the Red Sea territories; and nothing to indicate that African rhinoceros were ever brought to Rome otherwise than by way of Egypt.

There is no evidence either literary or pictorial that *bicornis* was ever brought to Egypt or Rome. It seems probable that the geographical distribution of *bicornis* has

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[28] F. R. Rodd (now Lord Rennell), *People of the Veil*, p. 318. Seventeen centuries after Maternus' expedition *simus* was still common some four or five hundred miles south of Tibesti.

[29] The Gulf of Suez and the canal to the Nile seem not to have been used, at any rate by north-bound craft.

not altered appreciably in the last 2000 years except in peripheral areas owing to its destruction by man since the general use of firearms (30), and that it existed in the Sudan in the days of the Roman Empire in much the same area that it inhabited 100 years ago; that is to say East of a line running roughly s. by w. from Tokar through Kassala—too far from the White Nile for convenient transport to Egypt. Moreover it is more aggressive and dangerous, more elusive when persecuted, and I think more difficult to handle unless caught when very young, than is *simus*. It seems unlikely that the Romans would wish to import *bicornis* if they could get the larger and more impressive *simus* with less trouble.

I think that after considering all the items of evidence set out above we are justified in believing that the classical rhinoceros was the White Rhinoceros as we know it today, and that it came from the Nilotic Sudan, probably from the West bank of the White Nile where (though much further South than in Roman times) it still survives in larger numbers than elsewhere (31).

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[30] The persistence of *bicornis* is illustrated by the fact that when it was found necessary recently to eliminate it from an area of some 600 square miles of dense bush, which was required for native settlement not much more than 100 miles from the capital of Kenya Colony, not less than 1000 rhinoceros had to be killed.

[31] There may be at most 2000 *simus* still alive in Africa, of which about half are believed to be in the Bahr-al-Ghazal province of the Anglo-Egyptian Sudan.

Moas and Man

(PART II)

THE WAIRAU MOA-HUNTER SITE

by Roger Duff

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BY reviewing the bearing on Maori culture history of the excavation of Moa-hunter sites in New Zealand the previous chapter serves as an introduction to this account of the Wairau site, which is the most important Moa-hunter site yet recorded.

So haphazard is the progress of archaeology in New Zealand that although this camp occupies more than 15 acres and is situated on an accessible beach about seven miles from the town of Blenheim, it was not discovered until 1939. It was first ploughed about 1922, when the tenant, Mr C. Eyles, was surprised at the number of bones uncovered. Being unfamiliar with moa bones, he believed the larger bones were those of the bullocks formerly employed to cart wool along the boulder bank, and the smaller to be human. Presumably miscellaneous artifacts were also revealed by the plough but only a number of stone adzes were recognized and retrieved. Of these the only one saved is a massive example of Type 1, A one of the most distinctive Moa-hunter types.

The site was not identified as of Moa-hunter age until some seventeen years later, when Jim Eyles, the thirteen-year old grandson of the former tenant, decided to emulate the spasmodic activities of local collectors who occasionally visited the site in search of 'Maori curios'. He opened up a trench on the edge of a convenient mound and immediately found the first of seven burials each, as subsequently established, with a moa egg grave offering.

The recovery of the perfect egg apparently overshadowed the fact that it had been blown from one end and was attached to a human skeleton. Long-standing beliefs, based on Maori tradition, that certain tubular beads of stone, normally found singly, had been used by their ancestors either as the fly-wheel of the pump-drill or as genealogical mnemonics, were however corrected by the recovery in the grave of seven essentially similar beads in whale-ivory assembled as a necklace and relieved by a sperm tooth as central pendant (Andersen, 1940). At best the find was regarded as a lucky accident with no reason to expect a repetition, no follow-up visit to the site was made, and it was left for Eyles in March 1942 to dig a little further into the same mound and uncover a second burial provided with the wealth of offerings shown in the plate. In addition to the egg, of which only crushed fragments remained, there were fourteen stone adzes, the largest 18 inches in length and weighing over 10 lbs., a necklace of eleven large moa-bone beads, a second comprising 18 moa-bone units worked to the shape of conventionalized whale-teeth, a third comprising 21 similar units, here cut from sperm whale-teeth, and approximately eight hundred drilled porpoise teeth.

A reconstruction of this burial as displayed in the museum is shown in PLATE I. It is essentially a conjectural restoration, based on no better first-hand details than Jim could remember a month later, with the depth and section based on my own observations on the third burial. Canterbury Museum did not intervene until after the discovery of this second burial and my field notes commence from Burial 3.

SIGNIFICANCE OF SITE

Before proceeding to refer to subsequent burials and to review the burial and midden recoveries, I propose to describe the situation of the Wairau camp.

In its location at a river-mouth and more particularly its placing at the extremity of the spit or boulder bank formed between the river estuary and the sea, the Wairau site agrees generally with the South Island east coast sites. Those recorded occupy on the average the mouth of every third or fourth river from the Wairau to the Clutha. For the selection of estuarine settlements there are obvious reasons. To settlers dependent on fishing and fowling*, landed by choice or accident on a predominantly treeless and barren coast, estuaries provide the greatest concentration of food. Assuming that the surviving moas favoured an estuarine habitat, this was an added incentive, and many Moa-hunter sites similarly occupy the dead end of a spit into which moas could be driven and readily trapped. (This placing would also accord with the alternative theory that the moas were rounded up far inland and brought down by water to these river-mouth sites, or even transported by sea from adjacent coastal localities. Inland Moa-hunter sites are however small, suggesting only transient occupation, and remarkably few ; up to now they have been recorded only in the Waitaki Gorge and the Maniototo Plain).

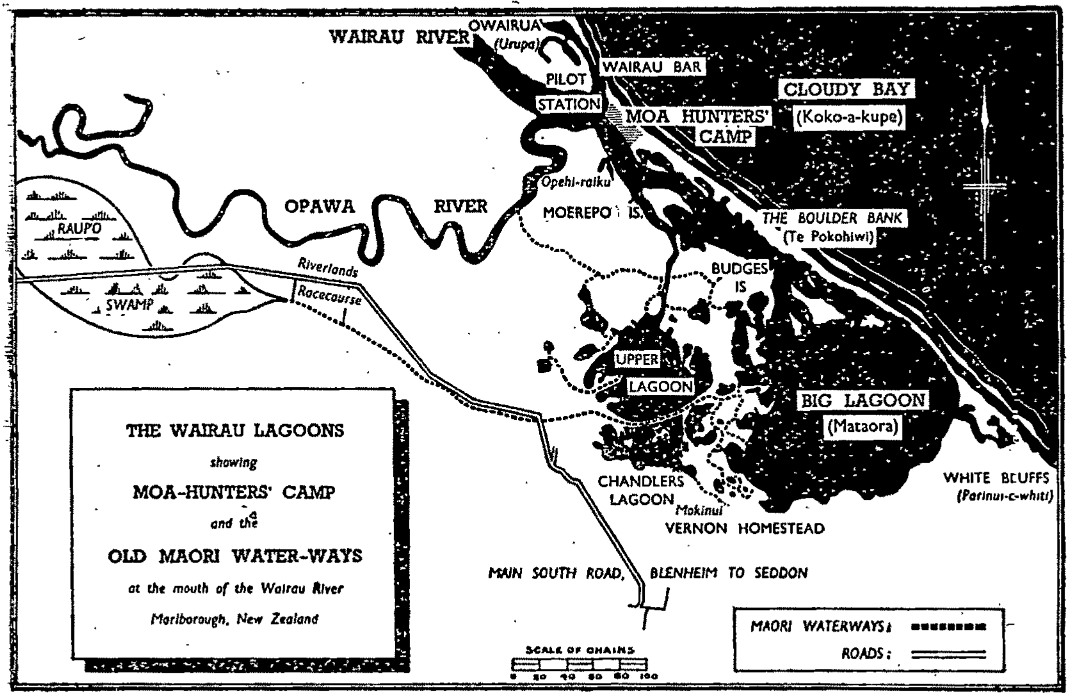

FIG. 1. LINE MAP, SHOWING SETTING OF WAIRAU CAMP

Certainly the Wairau camp is well sited for a moa drive, occupying the dead end of a long and narrow shingle spit, roughly 5 miles long and averaging 10 chains in width

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* Tradition is emphatic that food plants were not introduced until the Fleet migration (1350).

(FIG. 1). The spit furthermore originates in a narrow strip of sea beach closed off immediately to the south by the sea cliffs of the Vernon Hills which in turn follow the inner shoreline of the vast Wairau lagoons. Whether the moas were chased down from the Vernon Hills or cautiously edged round the inner lagoon shores it should not have been difficult for the Moa-hunters, and their dogs, to turn them up the boulder bank. Escape was then impossible and the birds were driven to the cul-de-sac of the river mouth and there despatched.

Here an area of 15 to 20 acres through which the plough or the spade uncovers midden moa-bones represents the end of the chase. Whatever hunting theory we accept, the concentration of bones represents in my view an abnormal accumulation. Were it a normal sampling of the moa population, their extermination, even by Cook's time, would seem incredible.

The outline map (FIG. 1) shows the position of the main camp (hatched) occupying the inner lagoon shore of the boulder bank extremity where the Wairau and its Opawa tributary enter the Pacific at Cloudy Bay. The broken lines indicate the course of canals, totalling 12 miles in length, dug out to provide channels through which migrating eels might be trapped. According to the Rangitane Maoris of 1912 these were only constructed in the 18th century, and we need assume no relation between these and the Moa-hunter occupation of the Boulder Bank.

It is important, in view of the persistence of Maori tradition that the pre-Fleet tribes had not introduced the sweet potato (*Ipomoea batatas*) the taro (*Colocasia antiquorum*), the yam (*Dioscorea sp*) and the gourd (*Lagenaria sp*), that the site appears unsuitable for cultivation. As the name implies the boulder bank is a narrow bar made up of millions of tons of beach stones and shingle thrown up by the sea. Even at the broader river-mouth end, where its width is 15 chains, it remains essentially a gravel and boulder deposit, deficient in soil. The same limitations to cultivation apply even more obviously to the remaining South Island sites, while south of the Opihi the climate would prohibit the growth of the sweet potato even if the soil were sufficient. Post-Fleet tribes succeeded, by taking great pains, in growing the sweet potato as far south as the Opihi river and to do so settlement was moved inland to richer and deeper soil. As this zone takes in the Wairau, Grassmere, Hurunui, Sumner and Rakaia camps one might expect, if the Moa-hunters practised agriculture, some trace of a Moa-hunter site on cultivable land.

It is noteworthy also that the Wairau site was not fortified, nor by 18th century Maori standards would such a site be capable of conversion into a strong point. Here again the other Moa-hunter sites agree. Taken in conjunction with the absence of the terraced fort (pa) in the Chatham Islands, this is presumptive evidence that the pa was developed in the North Island, possibly in the post-Fleet era.

The weight of (again) negative evidence is indeed against the assumption that the Moa-hunter tribes practised warfare or cannibalism. No human bones were found in the Wairau middens; nor any utilitarian implements (such as fish-hooks, needles, awls, etc.) in human bone. One necklace at Wairau comprises reel-shaped beads of what appears to be human bone; assuming the correctness of the identification, the conversion to amulets or ornaments implies that the bones were more probably those of relatives than of enemies. The absence of weapons is not conclusive, as the hand club (patu) of durable stone or bone may not then have been developed, and weapons might have been of wood which would not survive in open beach sites.

NOTES ON EXCAVATIONS. With traces of human occupation thinly rather than evenly scattered over an area 27 chains long and averaging 5 chains in width, systematic excavation has not yet been attempted. The most important exception was the burial area first discovered, where after the recovery over a three-year interval of Burials 1 and 2, the excavation was systematically continued as far as the area could be traced, and resulted in the recovery of a further five burials. Singly or in groups 24 further burials were found at widely scattered points, but in every case following ploughing. In addition to the range and wealth of grave offerings thus recovered, a considerable number of artifacts have been picked up on the surface after cultivation.

Although the oldest recorded, the site is thus typical of New Zealand in the shallowness of the deposit and in the absence of stratification. The burials from the cultivated area were in many cases struck by the share of a plough set at 9 inches. Assuming that breaking the surface of the light stony soil of the Boulder Bank had enabled wind erosion to lower the original surface, the greatest depth of any burial in an unploughed area (Burial 3) was only 28 inches. The Moa-hunter association of the burial finds is reasonably established from the evidence in nine cases of moa egg offerings, in some from the presence of moa bones in the grave and in others from the intrinsic evidence of the objects themselves. Similarly from the universal presence of moa bones in the middens the association of artifacts recovered by digging was not difficult to establish in practice. With surface finds after ploughing however the association can not be assumed, as such a site would be periodically visited and temporarily occupied until the early 19th century. So far however the nature of the surface finds warrants the assumption that the Moa-hunters lived there longest and in greatest numbers.

BURIAL CUSTOMS. The burial practice differed from that of recent Maori culture in several important respects. The favourite attitude, followed in all cases where the bones were those of an adult male, was extended or prone interment. A specific orientation (head to the east or south east and feet to the west or north west) was also consistently followed, and was departed from only with juveniles and females. In place of the complete exhumation and removal of the bones to a safe hiding place so commonly applied by the Maoris to men of rank, the relatives here were satisfied with the occasional removal of the cranium or the complete skull. This open burial adjacent to the village area with all or most of the bones and the grave offerings left intact suggests again that the Moa-hunters were under no necessity to conceal the remains of their dead from hostile intruders.

A typical burial (no. 5) is shown in line figure 2.

The skeleton, presumably that of a male, is prone and lying face downwards. It is complete except for the cranium, but the still articulated mandible indicates that the removal of the cranium was post burial. From the section it will be seen that the upper layer, demarcated by a heavy black line, shows clear evidence of disturbance at points immediately above the body and some four feet beyond the mandible where there is an accumulation of grave offerings. Most significant was the recovery of the units of a necklace of the whale-tooth type in moa bone. These were found ‘ twined’ round the butt of the large adze and the inference is that on opening the grave to remove the cranium the relatives lifted the necklace, still with cord intact, and thrust it into the far corner of the grave. The perforated moa egg is typically placed above the body and was apparently the last offering to be placed with the dead. Having survived the first filling in of the grave, and presumably a second disturbance and refilling, it is not surprising that it was found broken into scores of pieces, not all of which could be recovered.

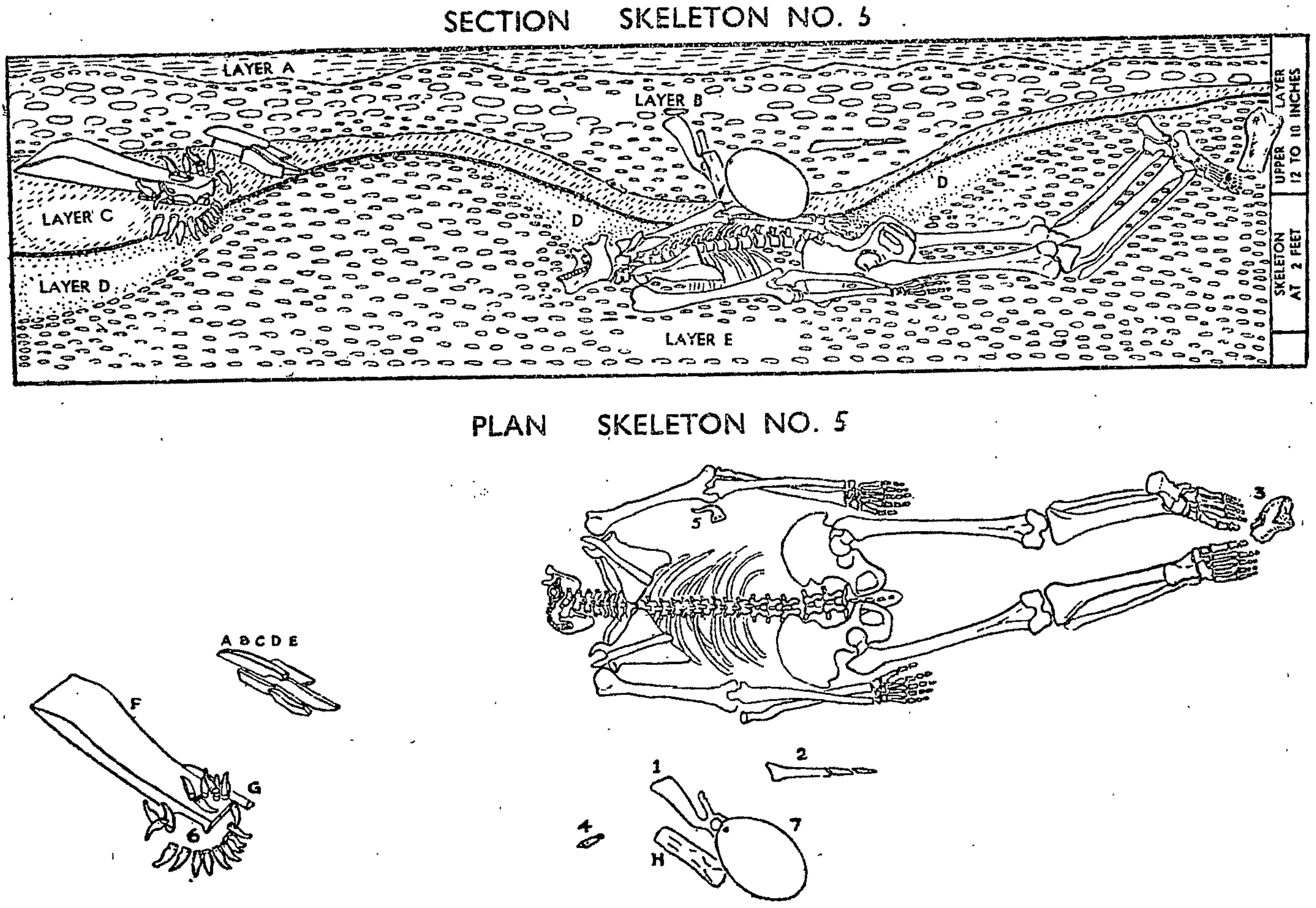
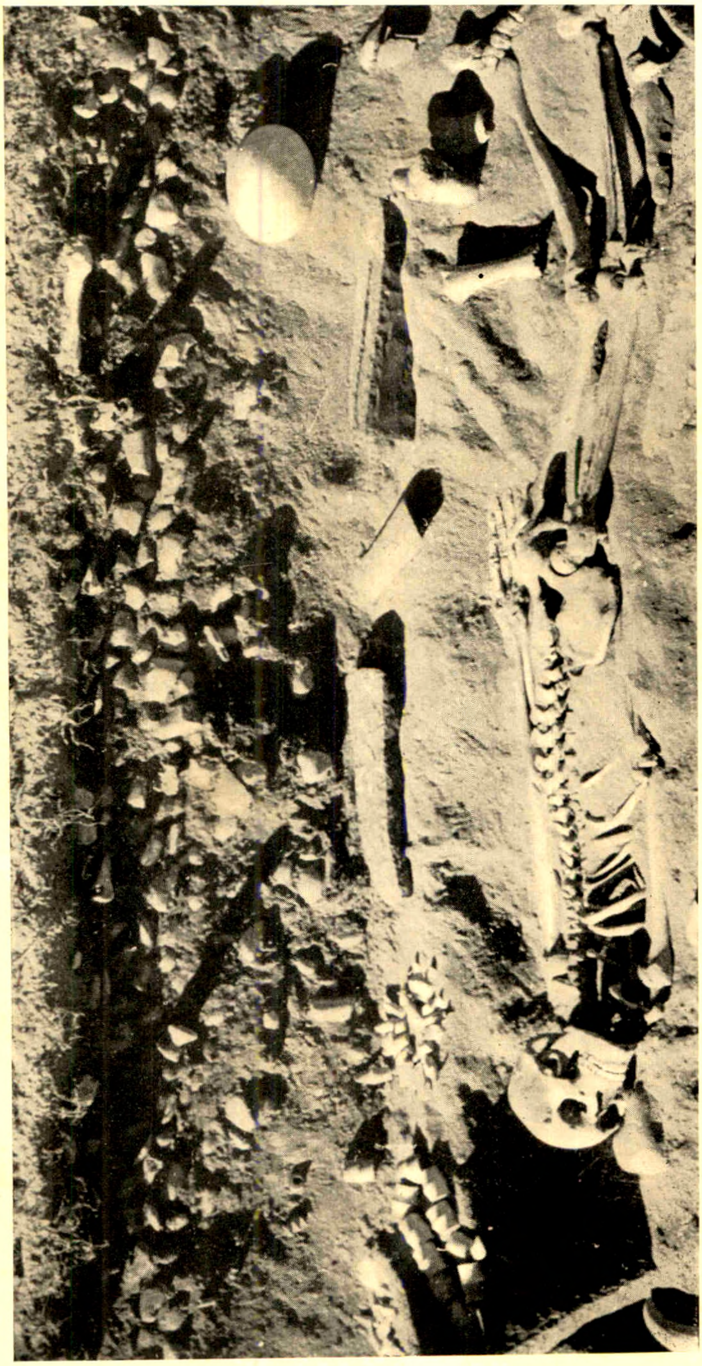
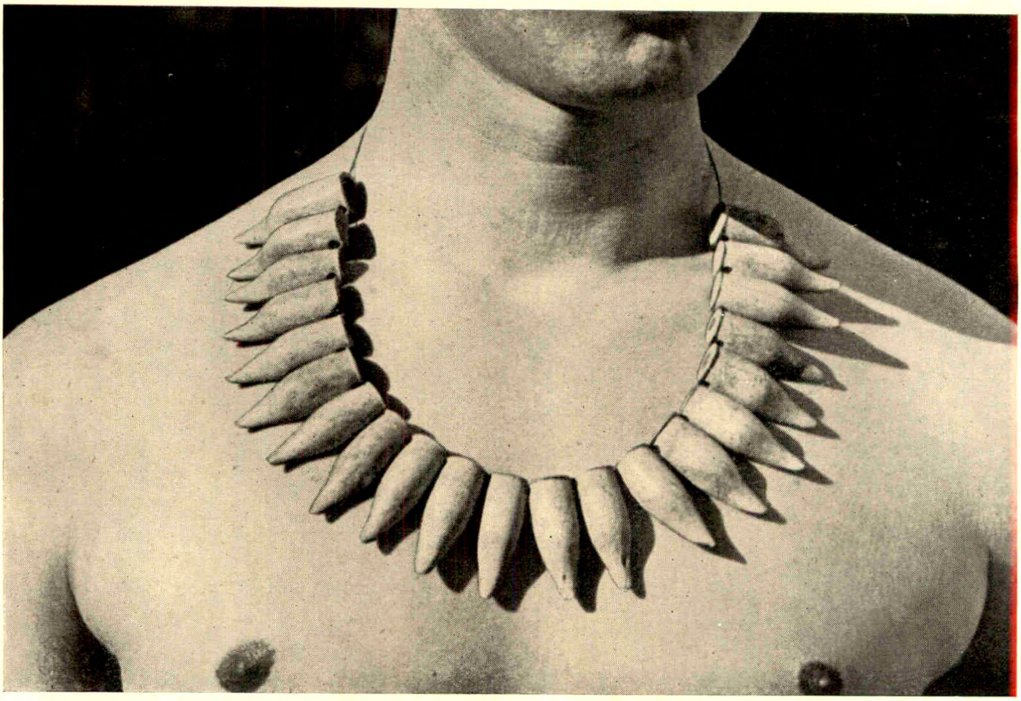


FIG. 2. LINE DRAWING OF TYPICAL BURIAL (No. 5) IN SITU.



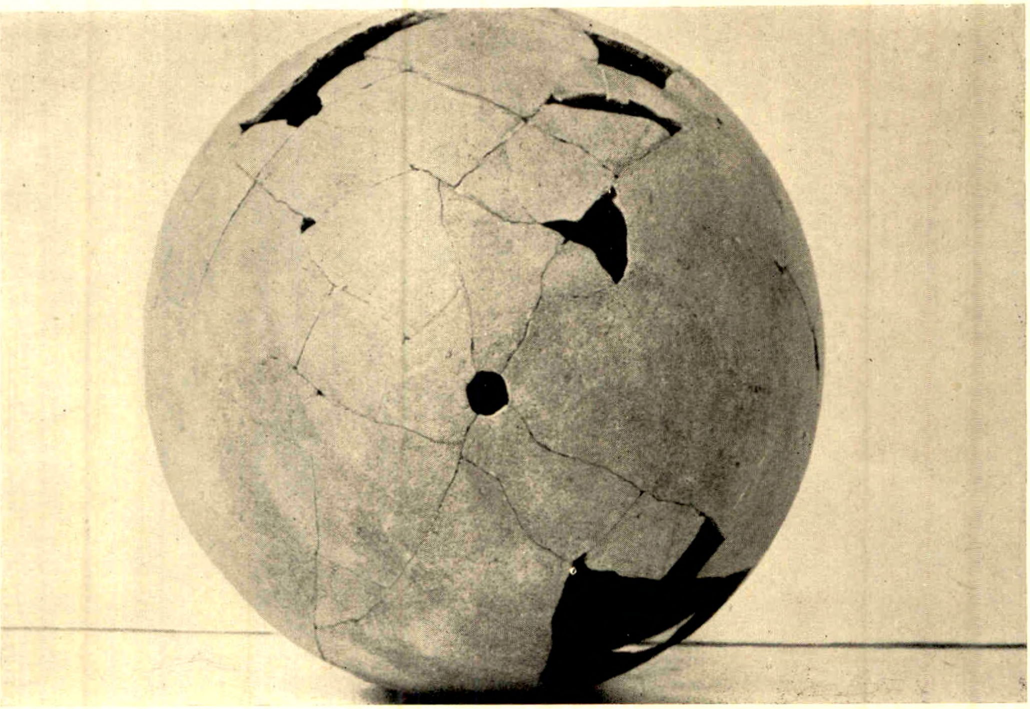
RECONSTRUCTION OF BURIAL No. 2, SHOWING GRAVE OFFERINGS, INCLUDING BLOWN MOA'S EGG.

PLATE II



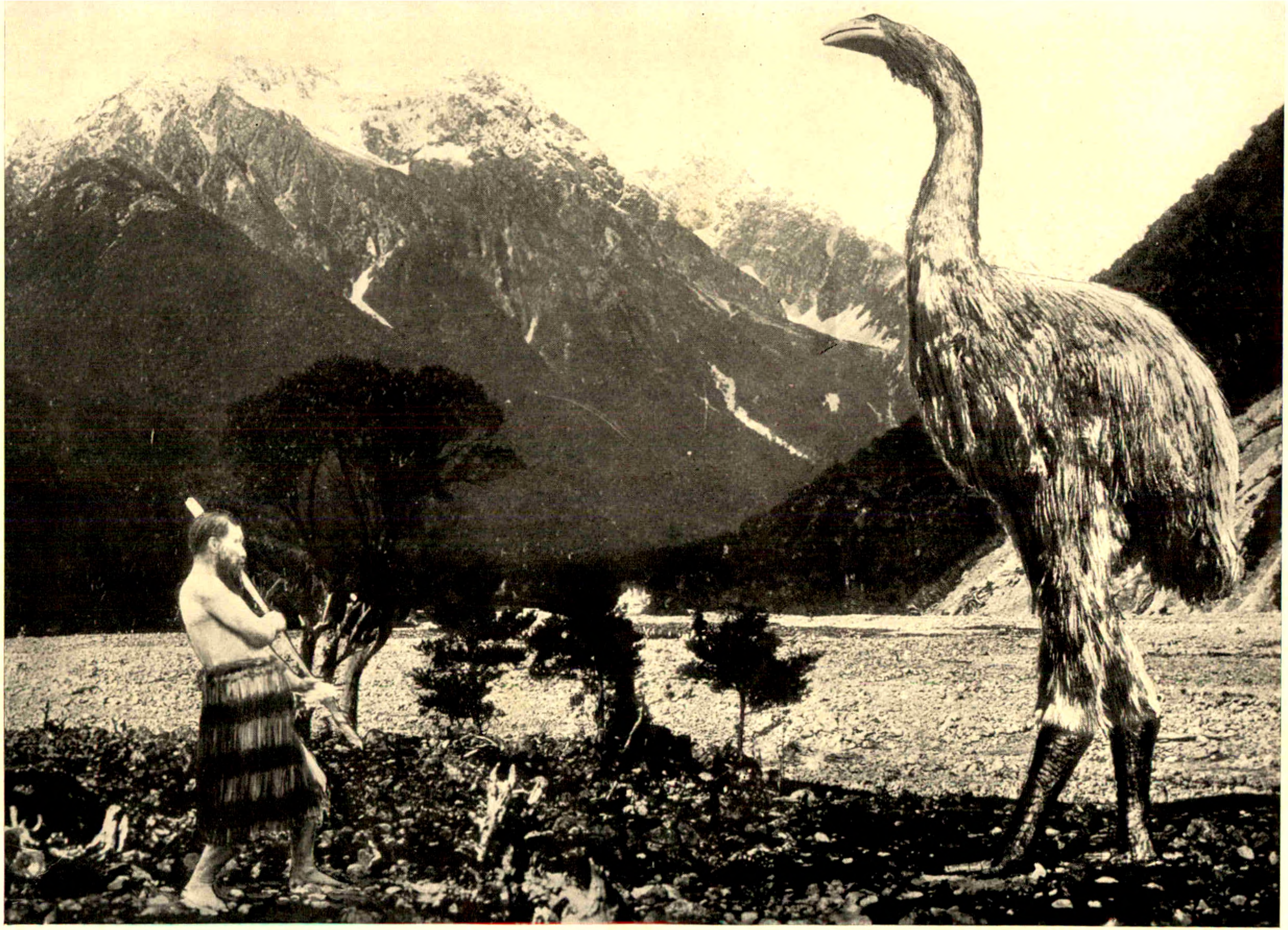
NECKLACE OF 21 WHALE-TOOTH UNITS IN WHALE IVORY, BURIAL No. 2

PLATE III



END VIEW OF RECONSTRUCTED MOA'S EGG FROM BURIAL No. 5, SHOWING PERFORATION
Max. width of egg 145 mm.

PLATE IV



RECONSTRUCTION OF A MOA

This reconstruction represents DINORNIS, the largest moa, whose extinction is generally accepted as pre-human. The alpine background is not typical of the habitat of the large grazing moas.

Photograph supplied by the High Commissioner for New Zealand

It was however painstakingly restored by Edgar F. Stead of Christchurch, and PLATE III clearly indicates the perforation and, incidentally, the perfect reconstruction of its contours.

The drawing (FIG. 2) also indicates another common occurrence, the presence of deliberately placed moa bones at burial level, here a fibula, the ischial process of a pelvis, and the proximal portion of a femur. Of particular note among the other offerings is the large perfectly finished ground adze (F) of the tanged quadrangular type with twin poll lugs. This handsome adze measures 450 mm. (18 inches) in length, 53 mm. in depth, while the cutting edge measures 136 mm. (5⅜ inches). The burial depth, here in unploughed ground, was 24 inches. The section could be divided into an upper layer with subdivisions respectively of surface soil (A), close packed stones (B), discoloured sand (C), varying in depth from 12 inches to 18 inches, and a lower layer of coarse shingle (E) with a shallow stratum of sand (D) adjoining the base line of the upper layer. The bones, egg and other perishable grave finds owed their state of preservation to the slight sand stratum, a circumstance even more marked in Burial 2 (see PLATE I). Here an upper layer of much the same constituents and depth rests on a solid zone of sand. By contrast Burial 29 was represented only by the crumbling skull and the faint remains of the long limb bones, a condition due probably to shallower burial (skull at 12 inches, feet at 17 inches) beneath a permeable matrix of loosely piled stones. It is noteworthy that the solid root portions of the 38 sharks teeth found *in situ* as a necklace had completely disintegrated. The contrast between the burials however more obviously indicates differences in the protective nature of the matrix than differences in age.

Owing to the frequent post-burial removal of the skull, and to the number of cases where the pelvic bones had disintegrated, I list with diffidence my determination of the sex of the burials, as follows ; adult male 16, adult female 3, immature 4, unknown 5.

The few measurable crania were examined in February of last year by Dr H. L. Shapiro of the American Museum of Natural History during the session of the Seventh Pan-Pacific Science Congress. At the risk of anticipating Dr Shapiro's report I record his opinion that their characteristics were strongly Polynesian, with few apparent differences from the Maori norm.

Evidence of inclusion of a moa egg was found in nine out of twenty-nine burials, including every burial of the first group of seven whose other grave offerings indicated that they were men of rank. The only two eggs recovered intact were each perforated from one end, while of the four capable of restoration two were perforated. It is clear that the eggs had been blown when fresh and the purpose of the single perforation was to convert them into water bottles. Assuming that the Moa-hunter occupation was prior to the introduction of the gourd, such receptacles would become valued property and their inclusion in the graves can be ascribed both to this value and to the tapu of their association with persons of rank. The masses of fragments of eggshell found throughout the area give evidence of a reckless use of the eggs, and this would be a major factor in the extermination of the birds. The eggs range in length from 194 to 207 mm., and in greatest breadth from 139 to 145. Falla refers them all to *Euryapteryx gravis*, the most numerous genus surviving in the human era.

Of bones of extinct birds, the moas could mostly be referred to *Euryapteryx**, a short, squat genus, with a few bones assignable to *Emeus huttoni*, a still smaller and less massive type. There was no evidence of the presence of Dinornis (sensu stricto). Bones of the extinct swan (*Chenopis sumnerensis*) occurred as regularly as moa bones and in direct association with middens. Bones of the extinct eagle (*Harpagornis moorei*) comprised a claw and several tibia and ulna ends worked into awls. The extinct crow (*Corvus moriorum*) also shared a contemporary presence in the middens.

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* Euryapteryx gravis (Owen). Synonyms: Dinornis gravis ; Emeus parkeri ; Emeus boothi ; Emeus haasti ; Euryapteryx kuranui.

Of mammalian bones, human bones were found only in burials and in one probable identification as units of a necklace of reel-shaped beads. Seals and whales were most numerous, while the Polynesian dog and the rat were certainly contemporary.

Shellfish comprised existing species.

Hooks and Harpoons. The importance of line-fishing in the Wairau economy was well shown by the numbers of hooks recovered. These were either bait-hooks conforming to the usual Polynesian shape, with inturned unbarbed point, fashioned in one piece from moa bone or the ivory of seal canines. The average size did not exceed two inches, so that we can assume that sharks and larger fish were taken with wooden hooks which have disappeared. The lure-hooks represented an exact copy of the Polynesian bonito lure, with the necessary difference that the shank was shaped in stone. This copy was probably not very effective because of the lack of a suitable substitute for pearl shell, and it was abandoned in post-Fleet times. The points of three small bone and ivory harpoons found agreed exactly with those of the Marquesas and were probably similarly operated (harpoons are unrecorded elsewhere in Polynesia).

The recovery of awls shaped from stout limb-bones of birds and the Polynesian dog, and bone needles occasionally found secreted in the awls, suggests that at this period considerable use was made of seal and bird skins (including those of the moa) in fashioning garments. A single example of the barbed bone bird-spear point, and three serrated blades of tattooing chisels, close the list of miscellaneous bone artifacts.

Personal Ornaments. The significance of the personal ornaments has been foreshadowed, in referring to the burials. They are of interest in reflecting the original patterns brought from Polynesia, before they were supplanted by the new forms evolved in nephrite. By contrast with the recent Maori ornaments which were worn as ear pendants or single breast pendants the most valued Moa-hunter ornaments were worn as necklaces. In one type the units represented smaller conventionalized copies of sperm or killer whale teeth as worn in recent Fiji. The Moa-hunter units were small, between 2 and 3 inches long, and considerably stylized. They were most frequently fashioned of moa bone, and the prevalence of moa-bone would assist the development of the fashion to a degree impossible in tropical Polynesia where it depended on the teeth of stranded whales. Of four necklaces found, the number of units ranged from 18 to 23. In one necklace of 21 units, each was fashioned from sperm whale ivory (PLATE II), but ivory units were otherwise exceptional. The closest resemblances from outside New Zealand are found in recent Fiji, while in the single breast ornament of Hawaii the treatment of the lower extremity agrees more closely with the Wairau form. This suggests not so much a direct influence from Western Polynesia as the retention in New Zealand of a form antedating the cultural differentiation of Polynesia into Western and Eastern areas.

The Polynesian affinities of the tubular beads of the second necklace type are again somewhat generalized. Recent Polynesian records are rare and of uncertain provenance, the evidence comprising a few strings of ivory beads, variously ascribed to Fiji-Tonga, the Cook Islands and Hawaii. In New Zealand the fashion was firmly established in moa-bone, then transferred to seal and whale teeth, to the fossil *Dentalium* shell, and to serpentine, limestone and other soft stone. The most impressive beads or reels, were those cut from the mid-femur of *Euryapteryx* ; these measured approx. 2½ inches long by

1½. The rounded mid-shaft needed elaboration only to the extent of filing a transverse median ridge and grinding the ends flat. Necklaces comprising respectively eleven, seven and four moa-bone units were found with the burials. The units were arranged round a central pendant comprising a complete sperm-whale tooth, for which in one instance an elaborate substitute in serpentine stone was provided, approximately 9 inches long, and weighing 1½ lbs. Ivory beads were rarer, and the sole necklace of 7 units was found with Burial 1. Only one example of the serpentine version of the reel was found, fortunately in a grave with moa egg offerings, demonstrating that this substitution originated when moa-bone was still plentiful. Stone reels were widely found in New Zealand before Wairau demonstrated that they were copies of easily made bone and ivory prototypes, and the complete lack of Maori knowledge of their purpose is well revealed in earlier discussion on them. Stone reels are evenly distributed between North and South Islands, although those of bone and ivory predominate in the latter.

Other ornaments comprised drilled porpoise teeth, and necklaces of pierced valves of a bivalve (*Myodora striata*), and 38 shaped teeth of the *Carcharodon* shark. As in the Chathams, the root apices were ground flat, to enable the teeth to ' ride ' better, and the perforations made through the root. Several large curved needles, worked from unidentified rib bone, appear identical with the cloak-pin of Maori and Moriori.

STONE ADZES AND CHISELS. The most numerous and important finds were the 207 stone adzes recovered (64 from the burials, 143 from the midden area). These not only make it clear that the Moa-hunter Maoris migrated from Eastern*, rather than from Western, Polynesia, but pinpoint the dispersal centre as the Society Islands.

The adze evidence is important enough to warrant a digression.

Eastern Polynesia is distinguished from Western by the presence of adzes with a deliberately provided lashing grip or tang, conforming respectively to a quadrangular (Type 1), reversed triangular (Type 4), and triangular section (Type 3), of which the first two predominate in marginal areas such as Hawaii, Marquesas, Pitcairn and the South Island of New Zealand, and the third in the central groups of the Society, Cook and Austral Islands. Adzes are only tangless when too small to make it easy to reduce a grip, and typically of quadrangular section with the planes of the back, front and sides sharply intersecting (Type 2, A).

The major and puzzling exception to this pattern of distribution has been the north Island of New Zealand. Here tangless adzes predominate (Type 2, B) in examples of every size, while the cross section is rounded quadrangular as in Fiji and Tonga (the ' Walzenbeil ' of Heine-Geldern). The problem has been whether the North Island type represents an early (Melanesian) migration via Fiji and Tonga (a highly plausible route geographically), or an example of the local development which Buck (1944) has shown to be widespread in Polynesia. The Wairau site demonstrated that the Eastern Polynesian types, which are characteristic of the South Island, reached there in the earliest known New Zealand culture period, while among the 207 adzes only 2 corresponded, imperfectly, to the North Island norm (Type 2, B). This supports the hypothesis that Type 2, B represents a local and comparatively late development originating in the North Island, the environmental factor being the widespread presence of older non-basaltic rock unsuitable for the skilful flaking necessary in shaping the Eastern Polynesian proto-types from fine grained basalts and clink-stones. In Fiji the same environmental factor was present, as well as the cultural influence of Melanesia where adze sections are always rounded.

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* Eastern Polynesia is the vast area of which New Zealand, Hawaii and Easter Island are the limits. It includes the Kermadecs, the Cook Islands (taking in the Northern Cook Atolls), Society Islands, Australs and Rapa, the Tuamotu Atolls, Pitcairn and Mangareva, and the Marquesas.

At Wairau the predominant material was a fine grained altered mudstone (baked argillite), a remarkably tractable medium, quarried from a few outcrops peculiar to the northern end of the South Island, in Nelson and Marlborough. The virtual absence of nephrite is striking, in the light of our knowledge that it is peculiar to the South Island, originating mainly at Arahura on the west coast. Presumably the early settlers favoured the eastern grasslands as the main habitat of the moas, and several centuries elapsed before nephrite was discovered and its possibilities realized. Only 2 of 64 grave adzes from Wairau were of nephrite, while the material had not become common enough to give rise to the Hei tiki, ear pendants and other nephrite ornaments characteristic of later Maori culture.

The major Wairau adze types will be briefly listed and described.

TYPE 1: (TANGED QUADRANGULAR). VARIETY A.

The cross-section is quadrangular, with all planes intersecting sharply, front somewhat wider than back. The grip is sharply reduced below the level of the blade which meets the butt in a marked shoulder; longitudinally the grip is somewhat concave, with the outer corners of the poll occasionally eleborated as lugs. The back is often strongly concave longitudinally.

The more common variety (40 examples) agrees closely with the standard Hawaiian type, differing in the evidence of reduction of the grip by bruising, a technique which foreshadows the elaboration into lugs of the outer corners of the poll. These are large adzes, ranging up to 467 mm. ($18\frac{9}{16}$ inches) in length, and 10 lbs. in weight. Within New Zealand the variety is common in the South Island, rare and localized in the North. Beyond New Zealand it is recorded from: Hawaii, Marquesas, Tuamotu, Pitcairn, Rapa, Rarotonga and Tokelau.

Of the more specialized variety with lugs on poll, only three were found, the largest, from Burial 5, obviously a ceremonial adze of great value. It was perfectly ground on all surfaces except the grip and showed no signs of use; length 450 mm. (18 inches), width cutting edge 136 mm. ($5\frac{3}{8}$ inches), weight 11 lb. 13 ozs. Within New Zealand this variety is sharply confined to the South Island and the Chathams. Beyond New Zealand it is found in an arc extending south and west from the Society Islands:—Raivavae and Rapa; Mangaia and Rarotonga; Nassau and Manihiki; Uvea.

Plotted on a map the distribution pattern of both varieties of the type shows a very clear diffusion from the Society Islands, and, to judge from the distance travelled from the dispersal point, an early diffusion. With the exceptions of the Tokelaus and Uvea the pattern sharply coincides with the Eastern Polynesian culture area and language family. The evidence from Western Polynesia indeed comprises only two adzes, which may indeed have reached there in post-European times.

Until recently no example of either variety was recorded from the presumed dispersal point, the Society Islands. Having searched in vain in museum collections in Paris, London, New York, Honolulu, Cambridge, Oxford and Edinburgh, I was startled to find last year a perfect specimen of the horned variety in the museum of the Whitby Literary and Philosophical Institute. This agreed so closely with the New Zealand forms that without its well attested origin from Borabora in 1830, I would have assumed it was from New Zealand. This important record suggests that examples of both varieties, although overlaid in the Society Group by the later triangular type, will be readily recovered by excavation and further search in existing collections.

The type is peculiar to the Oceanic area, and until recently was recorded only from Eastern Polynesia where it was considered by Linton (1923), Burrows (1938) and Buck (1944) to be a local development. Remarkably similar types have now been recorded by Beyer (1948) from Luzon in the Philippines, with extensions in Formosa, and on the south Chinese coast at Hoifung.

Beyer ascribes the type to his Philippine Middle Neolithic (2250–1750 B.C.) and derives the Eastern Polynesian tanged adze culture from it, a point of origin and a date which both agree closely with our reconstruction of the early proto-Polynesian migrations from Indonesia.

Type 1, Variety B corresponds with Heine-Geldern's (1932) 'schulterbeil' a thin quadrangular sectioned blade, tanged by the lateral reductions of the butt to form a 'spade-shouldered' grip. Only two argillite examples and one in nephrite were found. The type is not important in New Zealand or Polynesia, and the chief interest is in the existence of the type in South-east Asia. Beyer ascribes it to the Philippine Middle Neolithic but notes its rarity, and regards it as originating in Assam or Burma.

TYPE 2: QUADRANGULAR ADZES WITHOUT TANG. (VARIETY A).

These are essentially smaller examples of Type 1, the grip being neither necessary nor easy to apply, except by the 'spade-shouldered' method, above. Numerically an important proportion of the total, the 57 found ranged in length from 3 to 6 inches, in weight from 2 to 11 ounces.

TYPE 3: TANGED TRIANGULAR (APEX DOWNWARDS)

It is significant that the triangular cross section, which in the 18th century was the dominant one in the central groups of Eastern Polynesia, was represented as a minority in Moa-hunter New Zealand. The section of the Wairau examples is so shallow that the grip is produced mainly by the reduction of the sides, while the width of the blade at the shoulder exceeds that of the cutting edge. The only common form, Variety C, totalled 17 examples. Its characteristics are still retained in the most widespread variety of the triangular adze of the Cook Islands, and the chief interest of the New Zealand forms is in establishing the order of diffusion of the types.

TYPE 4: TANGED, TRIANGULAR (BASE DOWNWARDS)

The outstanding characteristic of Variety A is the triangular cross-section, with the base line the broad base or back of the adze, and the narrow cutting edge terminating the hog-backed ridge of the front or face. The cross section is only triangular towards the grip, and towards the cutting edge is sub-rectangular or trapezoid. There is a well-marked grip, reduced below the face to leave sharply projecting points at the poll and butt shoulder. The line of the back is often concave longitudinally.

Examples are of medium size, the largest of 47 found was 252 mm. (10 inches) long, with cutting edge 35 mm. (1⅜ inches), weight 2 lb. 13 ozs.

The shape clearly reflects a specialized purpose although the distribution cannot be explained in terms of that purpose. In New Zealand the distribution correlates with 1 A, being most common in the South Island, present but not common in the Chathams, rare and localized in the North Island. Beyond New Zealand it can be readily traced from the Society Islands, where it is not uncommon, to the Marquesas, Tuamotu, Pitcairn, Cook Islands and Kermadecs, a clear diffusion pattern.

This is a modification of the 'vierkantbeil' of Heine-Geldern and the 'keeled trapezoid ' of Beyer, by both assigned to the late or full Neolithic of Indonesia, and dated by the latter between 1750 and 1250 B.C.

Variety C and D represent a number of small gouges or rounded chisels derived in my opinion from a hog-back pattern, although often with rounded section, and typically without a grip. They reflect the great development of wood carving in the Polynesian Neolithic and remind us that the practice of fine carving in New Zealand is as old as the Moa-hunter era.

TYPE 5 : LATERALLY HAFTED ADZE

The final Moa-hunter type represents what I consider the most remarkable specialization in Polynesian Neoliths. Whereas the Polynesian adze was normally placed base downwards on the foot of the haft, with the line of the cutting edge at right angles to the axis of the shaft, this adze developed from a blade placed side downwards on the haft with the line of the cutting edge in the same plane as the axis of the shaft. One side thus became the base and the other (flaked to a ridge-like edge) became the face or front, a reduction at the butt providing a lashing grip. The implement still remains an adze in that the bevel retains its original position as on an adze resting upright on one side.

Of the 36 New Zealand records of this remarkable type, 28 came from the South Island and 8 from the North. Wairau produced three.

Beyond New Zealand the only true equivalent comes from Pitcairn. The Mangarevan ' axe ' is evenly bevelled to a wedge-like edge and is a true axe. In the Society Islands and other groups where it might be expected the need of altering the plane of the blade was met by mounting a normal adze in a wooden sleeve which rotated on the foot of the haft. This type would seem to be a purely Polynesian invention, and not, like the other types listed, an echo of proto-types in South-east Asia.

Conclusions from Adze Evidence

With the type material dated by the facts of excavation the Wairau adzes provide the most important evidence up to now on the origin of the earliest immigrants to New Zealand. They are seen to have arrived from Eastern Polynesia, and in particular from the dispersal centre of the Society Islands, carrying with them adzes whose proto-types are known from the Late Neolithic of Indonesia and Southeast Asia. The extent to which this immigration covered all New Zealand is unsettled, but the presence of the majority of the types in the northern extremity of the North Island, and their prevalence throughout the South suggest that the gap was caused by the local emergence of Maori culture proper in the centre of the North Island and the consequent retreat of the Moa-hunter culture to opposite marginal points. For the emergence of the later culture the chief social stimulus would be the landfall of all later migrations, down to the 1350 Fleet, on the North Island Coast. The environmental stimulus was the new and completely different physical nature of the land.

A Nineteenth Century Moa

In these articles I plead guilty to using the term moa very loosely, in some contexts to group all the widely differing genera of the Dinornithiformes, but more often with specific reference to the medium-sized eastern grasslands genera (notably Euryapteryx gravis) whose wholesale slaughter characterizes the Moa-hunter period of culture as defined here. It is to Euryapteryx that the traditional name moa presumably refers and I follow Falla (1942) in regarding it as unproven that the Moa-hunters' quarry included the gigantic Dinornis in the specific sense.

Since Part II was written strongly circumstantial evidence has been obtained of the 19th century survival of Megalapteryx, the small kiwi-like, forest-dwelling, genus which is technically classified with the Dinornithiformes. The discovery was made west of lake Te Anau in a subalpine valley in the only partly explored southwestern fiordland of the South Island, where a colony of the large flightless rail Notornis Hochstetter (or Takahe) was found surviving in 1948, fifty years after the last record of its existence. While patrolling Takahe valley in October last year Mr K. Miers found limestone shelters with evidence of recent occupation by a Maori fowling party. Investigating the surface dust of the floor in December, Dr R. A. Falla found well-preserved tussock bedding and textiles, including flax nooses and sandals, unfaded feathers of Kakapo, Notornis, Kiwi and ?Megalapteryx, and, belonging to the same occupation, the broken and dismembered skeleton of an individual of Megalapteryx. With Dr H. D. Skinner of Otago museum I agree that several deep cuts on the ischial process of the pelvis were probably chopped with a metal cutting edge, and local history would place the post-metal but pre-settlement period when the Maoris had access to such tools as between 1813 and 1840.

It is significant that the Southland Maoris have no traditional record of this encounter between Megalapteryx and man. While it is possible that this 19th century expedition represented the first penetration into the only survival pocket of Megalapteryx ever disturbed by man and the Maoris killed a bird for which they had no name, it accords equally with the absence of tradition that the Maoris did not recognize Megalapteryx as the moa of tradition and described it as a 'big kiwi' or 'giant rail'.

The Ngai-Tahu fowling party who ate Megalapteryx do not however qualify as Moa-hunters as this essay defines them. While for the sake of precision the latter might more strictly be called 'Euryapteryx-hunters' I propose to continue the more euphonious and still useful term Moa-hunter to refer to the demonstrably remote period when the moas survived in the eastern grasslands in large enough numbers to define and determine a well-marked Maori culture period centred on hunting them. For the North Island this period is terminated before the Fleet arrival of A.D. 1350, for the South I allow an overlap until A.D. 1500.

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The Apulia Expedition: An Interim Report

by J. S. P. Bradford

THE first (1949) season of archaeological reconnaissance on the Foggia Plain in South Italy has confirmed, in the most striking manner, the existence of one of the densest concentrations of ancient sites to be identified in Europe in an area of comparable size. For readers of ANTIQUITY, their nature had already been foreshadowed in these pages (1).

These discoveries were first made from British air photographs taken in June 1945, which revealed the plans of settlements, farms, roads and field-systems existing below the surface of the ground or clearly visible to the air camera. They were distributed across 3000 years of Italian history, from the Neolithic to the Middle Ages, and illustrate from the archaeological record three principal stages in the rise of a European peasantry. This is a theme that affords ideal ground for the conjunction of Archaeology and Ethnology; and it is appropriate that the Expedition should be based on the Pitt Rivers Museum at Oxford, which was founded on this very principle.

Now, thanks to the Apulia Committee, set up under the auspices of the Society of Antiquaries and representing the University of Oxford and learned societies, it has been possible to proceed to the second and fundamental stage :—namely, to carry out the first systematic programme of archaeological investigation based on air photographic data ever conducted in Italy. This also included the first British excavations there for a number of years, with the permission and helpful co-operation of the Italian authorities (2), and the support of the British School at Rome.

Our target was a coastal plain some 60 miles long by 30 miles wide, rather larger than Wiltshire and with a soil as rich in sites, but in a large measure unexplored. In 1949 more than 50 new Neolithic, Roman and Medieval sites were measured and examined on the ground, and several were also tested by trial-excavation in a season lasting only 10 weeks, beginning in September. This selection is only a small proportion of the sites revealed by the photographs, but they were distributed over the area from one end to the other. The detailed clarity of their plans on the photographs makes it possible to combine speed with the greatest precision in locating them. Field research of this kind and on this scale is, therefore, a unique departure in Mediterranean archaeology. British archaeology has a fine tradition of taking the lead in developing this special technique, and there is now an unequalled opportunity of bringing this experience to bear on the study of the ancient civilizations at the heart of the Mediterranean world.

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[1] ANTIQUITY, December 1946 and June 1949.

[2] We wish gratefully to acknowledge the valuable help received from His Excellency the Italian Ambassador, and Dr Montanari of the Embassy; from the Director-General of Antiquities and Fine Arts in the Italian Ministry of Public Instruction (Dr G. de Angelis D'Ossat); from the Superintendent of Antiquities for Apulia (Prof. C. Drago) who greatly facilitated the work; from Sig. Argardio Campi and his staff; and from Sig. Fernando Pavoni.

My particular thanks are due to Professor C. F. C. Hawkes, Mr I. D. Margary, Mr T. K. Penniman, and Professor R. E. M. Wheeler (Chairman of the Apulia Committee); and also to my wife for all her help before, during and after the Expedition and in the preparation of the plans.

There are no other regions in Italy in which the natural conditions of terrain and cultivation are so suited for this purpose. To put it into effect, grants were generously made for a preliminary season in 1949 by the Society of Antiquaries, the Trustees of the Leverhulme Fund, the Craven Committee of Oxford University, the Prehistoric Society, the Ashmolean Museum and by a donor who gave invaluable aid when most needed.

But this year, in 1950, a wider appeal has become imperative if the programme is to be carried through in full. Its curtailment would jeopardize the results already obtained, which depend for their full value on being systematically completed. A detailed Appeal for Funds has been issued through the Committee, and contributions which will be welcome, can be sent to the Apulia Committee, c/o the Asst. Secretary, Society of Antiquaries, Burlington House, Piccadilly, London. The work of the second season will complete the reconnaissance phase, and should clear the way for later work on a larger scale at one particular site. But this year's programme is, perhaps, the most vitally important. The survey of new sites of all periods must be continued (many fine ones necessarily remain unvisited as yet), and the test-excavations must be rounded off. Rapid and irrevocable destruction by ' deep ploughing ' machines, which with steel teeth tear up the rocky subsoil by acres at a time, is becoming widespread, and adds a note of urgency. One of the finest examples of a centuriated Roman field-system and road junction was utterly and forever obliterated, thus, shortly before last year's Expedition arrived.

The 1949 season divided itself naturally into two phases :—fieldwork came first and this occupied over 5 weeks, while test excavation followed at a few selected sites. These confirmed, beyond doubt, two fundamental points. (1) It was found that work on the ground confirmed (while naturally adding to) all the general conclusions already arrived at by the direct study of the photographs alone. (2) It was shown that, with the use of Photogrammetric Tables, extreme accuracy was possible in determining and measuring the exact position on the ground of crop-mark sites visible on the air photographs. Those used had often been taken from a height of two miles or more, and the object sought might be a small silted-up ditch, only a few feet wide and non-existent as an earthwork, indicated by a few wisps of vegetation in a dried, prairie-like expanse of stubble or sun-burnt pasture. In every case where excavation was carried out the expected ditches, walls, etc., appeared clearly with the first trench to be dug. Even when, as at Masseria Villana, the land had been recently ploughed and there was not the slightest hint on the surface of the twenty-foot wide Neolithic enclosure-ditch beneath, its position was located in a few minutes by measurements taken from the photographs, which gave its distance from two minor modern boundaries ; and the first trench revealed it. This would be equally true of comparable Roman and Medieval sites. *If* the photographs are taken and used with precision, we can now say that air photograph sites of all the main types are thoroughly reliable for archaeology on Italian soil. The establishment, in Apulia, of this important fact was one more proof that the basic principles of discovery from the air, worked out by O. G. S. Crawford, have a world-wide application under appropriate conditions.

In the late summer, circumstances are very favourable for fieldwork. The land is then clear of crops (which, though welcome from the air, restrict work on foot), and bears instead a rich and tell-tale covering of wild flowering plants, grasses and minor weeds. The results are often remarkable and sometimes dramatic, and as indicative of the plans of ancient sites as the crops earlier in the year. There is no doubt that ' weed-marks ' in Apulia (and, under similar conditions, in other regions and countries) are much more than a bizarre phenomenon. They form a distinct, recognizable, and (most important of all) very reliable class of archaeological site deserving greater emphasis. Broadly speaking they obey the same 'laws' as do crop-marks, but the weed-marks take a far greater variety of forms. Above ancient ditches flowering species grew taller, more thickly and often more quickly; and, thus accelerated, had sometimes flowered, withered and prematurely 'fall'n into the sear, the yellow leaf' before those not so placed. A score of different varieties of wild plants indicated the presence of ditches and other features beneath them. Very often Neolithic and Roman enclosures stood out as bold lines of colour, which could be faithfully recorded by colour-photography. Thus, during our 'ground-check' of sites seen on the air-photographs we frequently found Neolithic village ditches brilliantly outlined as broad bands of chrome yellow by flowering *Rugola* (a local name for a plant resembling wild cabbage growing in big clumps up to 3½ feet high). This plant, which is found everywhere on the Tavoliere, has roots up to two feet long that take advantage of the extra depth, and its superior growth thus reveals silted-up ditches of all periods impartially. The details of the Roman landscape were often delineated in flowers, by mauve wild mint, white 'cow parsley', or yellow-flowering thistles. The importance of these weed-marks has been stressed as they are a major factor in the field-archaeology of the region.

A brief summary of results achieved and waiting to be followed up will at the same time outline the nature of the plans for 1950, approved by the Apulia Committee:—

(i) *Prehistoric.* The air-photographs had revealed the buried plans of over 200 ditch-enclosed villages and smaller 'homesteads' on, and round the edges of, the Foggia plain, which previously had been a virtual blank on the distribution maps of such sites. With so much to be established the only possible policy had a faintly Elizabethan ring, i.e. to confirm their existence and assess their wealth. In a very real sense, an ancient world lay literally at our feet. After identifying a number of such sites by the cropmarks of their outer ditches and enclosures within, a typical example was chosen for test-excavation in which the plan was particularly clearly shown on the ground by well-marked growths of several species of weed. This site, near the farm of Passo di Corvo (7 miles N.E. of Foggia), was selected in order to establish firmly the Neolithic horizon already suggested for the whole group of large crop-mark villages. Enclosed by double and sometimes triple ditches, it covered an area 800 yards long by 500 yards across, within which are numerous enclosures that seem to fill it fairly completely. The position is only slightly raised above the surrounding plain, and is some distance from any existing stream. Our 'ground-check' made clear that the Neolithic farmers unerringly made use of every slight eminence when siting their settlements. Most were not positions of defensive strength, but of better drainage, etc.

The original photograph which led to its discovery has been illustrated and described in an earlier article (3). It shewed the lines of the 2-3 village ditches in the foreground and background, and some of the 100 circular enclosures so far visible within; at that date revealed by crop-marks. We chose a typical enclosure among those at the centre of the photograph (compare PLATE V, A). When thus directed to the spot the line of its ditch was seen to be clearly outlined on the ground in a strong localized growth of a particular weed, which exactly revealed its course, its width (almost to an inch) and its wide entrance gap. Guided by the photographs an immense economy in excavation was possible and the trench-grid immediately revealed the butt-ends of the ditches and all principal features.

The area enclosed measured 60 feet by 50 feet. The surrounding rock-cut ditch

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[3] ANTIQUITY, December 1946, plate III; and see site plan, *ibid*, June 1949, p. 63, fig. E.

was 4–5 feet wide and 8–9 feet deep, having vertical or undercut sides. Where excavated, the ditch had been cut as a continuous whole and not in segments or as a line of pits. Traces of a stone revetment were found along the inner lip, but a careful search revealed no definite structures or post-holes at the entrance or within, in soil or rock, in axial trenches 10 feet wide laid at right angles across the enclosure, supplemented by others. However, large fragments of sun-baked clay with 'wattle' impressions of thin branches were found in the ditch, and also in that of the small Neolithic enclosure near Masseria Villana (see below), implying the existence of structures nearby. The entrance gap was later increased from 18 to 36 feet by filling-in part of the ditch up to ground level, holding this soil in place with a facing of stone blocks built up in the ditch to form a new butt-end. There was thus preserved, below ground, an unusually good specimen of Neolithic walling. But the function of these enclosures is still obscure. Surface-draining is a possible explanation of their ditches, coupled perhaps with water catchment. We witnessed, here, the effects of autumn rains which turned the sun-baked ground into a quagmire and rapidly filled the ditch with 4 feet of water. Further attention will be directed to the problem this year; and, for comparison, enclosures of this kind in other parts of the village will be examined, in order to determine from the pottery whether the whole community of Passo di Corvo can be assigned to a single phase of the Neolithic. In 1950 the big ditches surrounding the village, each with a formidable weed-mark width of up to 20 feet, must also be sectioned; and, if time permits, the single ditch bounding the outer annexe (larger than the village itself).

The 'pilot' excavations of 1949 proved of great value in showing the range and prolific scale of the finds to be expected from these massive village-communities. This enclosure at Passo di Corvo yielded almost every typical item of Neolithic equipment (apart from figurines) including small polished and flaked axes, saddle querns (4) and polished stone-rubbers, bone points, and broken sherds fashioned and pierced to form an oval pendant and circular spindle-whorl. Characteristic implements and cores in chert (plus obsidian flakes (5)) included small blade fragments bearing the gloss that comes from use when set to form a sickle. There was no sign of any weapons. The great quantity of animal bones will provide good material for the identification of the species domesticated or eaten. The pottery was copious, with over 4,000 sherds from this test-excavation of 14 days. Its full publication must be deferred until further work is done, but the main elements are clear. The prominent class is that of plain carinated bowls, basins, open dishes and globular vessels with tall rims, beautifully burnished, thin-walled and symmetrical in glossy black and rich brown tints; and also very thin burnished bowls in tones of cream and pale grey. Besides these there are other fine wares fired a warm buff, and painted in broad tomato-red bands (*a fasce larghe*) or alternatively, though rarely, in red and white, or a dark chocolate shade in squares of lattice pattern. All of these maintain a very high standard of craftsmanship. One sherd alone had decoration incised *after* firing. Coarse, heavy, pedestal-bases were popular. Sherds decorated with impressed ornament executed with finger tip, shells, etc., were represented but not very commonly (shells from the sea, 15 miles away, were found in the ditch).

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[4] Also shaped tabular fragments of baked clay with a smooth upper surface, probably used in the preparation of food. These are unusual, but in the south Sudan oval grinding stones of baked clay (used with a wooden roller) are found today among the Nuer, in a region lacking stone (see Evans-Pritchard, *The Nuer*, p. 170, fig. 12).

[5] The volcanic mass of Monte Vultore, in the Apennines to the south west is the usually accepted source.

The decoration of a few was painted on one side and impressed on the other, showing the fusion of two traditions (6) of ornament.

Passo di Corvo ranks as probably the largest known Neolithic site in Europe (7), being over twice the size of Köln Lindenthal and Mayen, and appreciably larger than Windmill Hill, Michelsberg or Lengyel in Hungary. When its ditch-enclosed annexe is included with the main village, its length (1500 yards) surpasses even that of the great Neolithic fortified site at Urmitz on the Rhine. Nor was Passo di Corvo unique; other Neolithic villages of similar proportions were confirmed in 1949 near Masseria Palmori and Masseria Schifata. Size alone, however, is not all. Communities like these have an intrinsic importance for investigation, because they are the largest units which their society and economy could produce at that time, and consequently are likely to provide the most developed aspects of Neolithic peasant culture. We can see, too, from the pottery, that the Passo di Corvo series was derived from a Balkan source or sources (however peninsularized this became when once established in Italy) and the value for research of the Apulian group is further increased since the Balkan sites are for some time likely to be inaccessible.

The large Neolithic village sites at Coppa Cavone and Masseria Palmori, eight miles W.N.W. of Foggia, produced characteristic sherds from ditch-sections; and our trial trenches confirmed that the smaller circular ditched enclosure at Masseria Villana, typical of the ' homesteads ' of simple form so numerous on the photographs, were also Neolithic. Thus the characteristics of the ' First Farmers in Italy ' are growing gradually clearer and will, I believe, increasingly command respect as these foundations of Mediterranean civilization emerge. We have the opportunity of adding depth, detail and animation to what for long seemed a shadowy ' still-life ', and our new ' Landscape with Figures ' makes a better perspective background for the understanding of subsequent periods. The end of the Neolithic phase and the fate of the big villages remains obscure, and air-photography has not yet been able to pin down a distinctive type of Bronze Age site in this region.

(ii) *Roman.* We come now to the archaeological evidence for the state of Apulian peasant life in the Roman period. We were able, for the first time, to confirm the existence of many square miles of centuriated Roman field-systems, with their roads, private boundary ditches, and farm-enclosures, complete to the small trenches for vines and tree-pits (for olives, etc.) which had been revealed from the air by weed-marks and crop-marks above the ditches which outline them. In no other part of Italy, so far as is known, does there exist any comparable centuriated Roman landscape which has been safely preserved underground for excavation and direct study. In 1949, its traces were examined first by field-work over wide areas, lying miles apart, and next by test-excavation at one or two points.

Such an investigation of a Roman landscape broke new ground. Many of its features had never before been the subject of excavation, and thus we had to begin by establishing their typical appearance and variations. When armed with the air-photographs their outline on the ground is astonishingly clear, when the eye is thus directed to them. Never before have the details of the Roman farmer's landscape been so completely shown. More than a dozen kinds of wild plants and grasses display them well, so that they can be

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[6] R. B. K. Stevenson's periods I and II; see *Proceedings of the Prehistoric Society*, 1947, p. 85 *et seq.*

[7] Cp. Hans Lehner, ' Der Festungsbau der jüngeren Steinzeit', *Praehistorische Zeitschrift*, II (1910), 1–23; Childe, *The Danube in Prehistory*; Curwen, ANTIQUITY, IV, 22–54.

traced, measured and excavated. The initial problems were those of confirmation and identification, followed by the more difficult search for dating evidence. Trial excavation in 1949, within the field-system of *Luceria* (established as a Latin colony in 314 B.C.) was successful in obtaining a quantity of Apulian 'derivative-Hellenistic' black-glazed pottery from farm-enclosures, which were sited along the grid of centuriated roads that methodically partitioned the landscape. Test excavations must be completed at Posta di Colle, near Lucera, where there is a fine row of such farm-enclosures each connected to the road by a short drive. All such features are shown by the special growth of vegetation that reveals the ditches demarcating them. The extent of one group of farm-buildings placed round a courtyard, and the two building periods in its plan, were established in 1949. The pottery, now under review, indicates a period at least as early as the 2nd century B.C. for this block of fields, and further material from this year's work may well provide a closer date. We can probably never come closer than here to the realities of farming under the Republic. Certain features in the lay-out of the centuriation below *Luceria* also point to an early date (8), and it may well prove to be one of the earliest systems known. We are thus enabled gradually to supplement the scanty but tantalizing written record for the Romanization of this region of Italy.

In the southern half of our area, round Orta Nova and Cerignola, the encroachments of the excavations for modern vineyards and olive groves is permanently erasing most of the centuriated field system sited near *Herdonia* (Ordona). Several of the remaining fragments of its grid of crop-mark roads have been destroyed, since the air photographs revealed them, by this deep trenching into the rock which itself follows the Roman practice. Without this synoptic view over square miles of country given by the air photographs, these small isolated areas of crop-marks (on 'islands' of arable in a 'sea' of olives and vines) would never be recognized or pieced together into a whole system. As the new modern boundaries above ground naturally pay no attention to those below it, one can see clearly the way in which a whole region once covered by Roman centuriated roads can disappear without trace, and eventually fail to survive even in crop-mark form. Even on the huge expanse of grazing and arable land round Lucera, to which principal attention was given, one could never have obtained a coherent idea of the system there, as a whole, without the air photographs as guides. This is but natural, since the parallel crop-marks of the road grid lie hundreds of yards apart, on level ground without vantage points, and are often interrupted for long stretches by types of cultivation incapable of revealing the necessary marks.

Field-work supplied an answer to one problem which the photographs by themselves did not make evident. On the ground, it became clear that the large blocks of centuriated land (9) at the back of the plain were laid out to cover low plateaux, stopping short at their edge, and the axis of each block was adjusted to the natural orientation of the plateau so as best to fill the available area. The blank areas up to several miles wide between the systems, which were a puzzling feature on the photographs, were (we saw) caused by lower-lying ground which they did not choose to centuriate. The significance of such relatively slight topographical differences can only be appreciated on the ground.

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[8] E.g., the incorporation of earlier tracks as boundaries for the systems; see ANTIQUITY, June 1949, p. 67, fig. 2.

[9] These we hope to be able to equate with the land assigned and centuriated round the towns of *Luceria*, *Aecae*, *Ausculum*, *Canusium*, *Herdonia*, etc., mentioned in the *Libri Regionum*; cf. Pais, *Storia della Colonizzazione di Roma Antica*, 1923. Pais argued that the ascription of much of this to the Gracchan period was reasonable, although the absence (as was then believed) of surface traces had thrown doubt on all these entries.

The Roman vineyards lay along the centuriated roads, and the narrower private roads which ran off them. The lines of parallel ditches dug for the vines were very clear on the ground in various kinds of vegetation. In one area, 4 miles E.N.E. of Lucera a group of many vineyards (each distinguished by differences in the orientation and spacing of the vine trenches) extended continuously over an area of more than 130 acres. This was exceptional, but one big and compact Roman vineyard near Posta S. Cecilia, 4½ miles S.W. of Foggia, itself covered 44 acres. A more typical vineyard, near Masseria Villana between Lucera and Foggia, measured *c.* 200 by 300 yards, with an area of about 12 acres (though many of course are smaller). The foregoing examples all abutted on centuriated roads, and the vine-trenches were as clear on the ground (as weed-marks, etc.) as from the air, though more difficult to photograph. The excavation of examples on widely separated sites proved that the trenches for the vines were normally dug in continuous lines, being 3 feet deep from the surface (i.e. 2 feet deep in the rock), *c.* 1½ feet wide and straight sided. The surface crop-mark varied between 2½ and 4 feet in width. Trial trenches showed that they were set 8½, 9½, 11½ or 13 feet apart, according to the particular vineyard. At one Roman farm (near Masseria Villana), we excavated rectangular slot-shaped pits 4½ by 1½ feet and 3½ feet deep, sited alongside the ditches flanking the 'drive' up to the farm from the centuriated road. The effect must have been pleasing. This method corresponds to that more commonly used today for planting vines, in which pits of this size hold a pair of plants. One site, on a centuriated road 4½ miles S.S.E. of Foggia, was revealed in a particularly interesting way. The air photographs showed rows of Roman vine-trenches and tree pits. On the ground these were very clear where they crossed the rough bare surface of a broad medieval drove-way (*tratturo*). There they stood out as miniature banks and mounds, respectively, only 9 to 12 inches high. The roots of the small weeds which flourished above them had bound the surface soil and retained it when elsewhere this had been eroded by wind, rain, etc. Excavation showed that the Roman tree pits, cut well down into the rock (only 1–1½ feet below the surface), were 4 feet square, 3½ feet deep and flat bottomed (**10**). Modern Foggese farmers, when planting young olive trees, dig pits of standard size into the rock measuring 1 metre cube. In this instance, the spacing of 10–12 feet between the pits would have been too close for olives. However humble, archaeologically, such features of the Roman landscape may seem, these precise facts bear directly upon the staple produce of the Roman farmer and help towards establishing a solid basis for the economic history of the region.

In examining the centuriated roads, which were here excavated for the first time, we concentrated on the system laid out below the *colonia* of *Luceria* (Lucera). This extended for some 5 miles along a low plateau roughly running from east to west. At the upper (western) end rose the town on its hill, the hub of this little landscape. Among sections cut through these roads, a typical example was that near Posta di Colle (**11**), shown on PLATE VI. The road was 15 feet across, with square-cut flanking ditches 3½ feet wide and 4½ feet deep. There was no evidence of metalling, and it is doubtful if this ever existed. We were able to show that the many varieties of vegetation that revealed these ditches were

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[10] Near Posta di Colle, 4½ miles east of Lucera, excavation gave the same dimensions for other tree pits of this kind. These appeared on the ground as patches of extra tall wild plants. Lines of them were interspersed between the rows of Roman vine-trenches (as small fruit trees are today) in a neat 'plantation' along a centuriated road, directly opposite a Roman farm-enclosure.

[11] On the 'pasture' land on this abandoned airfield there were long continuous stretches of centuriated Roman road, each easily traceable for half a mile on the ground.

a reliable guide to the dimensions. Thus at Masseria Perazzo, nearby, we could calculate a width of 14 feet for the road and 4½ feet for the ditch on each side, here very clearly shown by the extra growth of 'fichi selvaggi' (a low thorny plant with pulpy red fruit), withered but still distinctive when we visited the site. The *Luceria* system is unusual in that only the east-west lines (*decumani*) of the normal centuriated grid of lines were laid out. A further important point lies in the spacing of the parallel roads. On the south margin of this system the last pair lie *c.* 790 yards apart, and the penultimate pair *c.* 580 yards apart. However, the remainder are regularly spaced at intervals of *c.* 580 yards apart, which is the equivalent of exactly 15 *actus*. Theoretical explanations of this are not obvious but there were probably solid practical reasons, and perhaps the lay-out was adjusted so as best to take up the area to be allotted. These and other departures from the 'blue-print' models of the Roman surveyors' text-books, with their neat gridded systems, and also from the commonest method of basing the land partition on *centuriae* 776 yards square (20 by 20 *actus*), make the *Luceria* system appear experimental or early, or both. We must give due weight to the fact that all these new Apulian systems, unlike (12) others (e.g., the examples in the Po Valley (13) which are so often reproduced to illustrate a centuriated Roman landscape), have the supreme advantage archaeologically that there is no living successor to systematize them and continually modify the original features. Until now we have had to be content with rigid skeleton-plans which seemed somewhat devoid of reality. With such systems, embodied in modern roads and boundaries, research could not normally pass beyond the elements of the geometry. It is all the more welcome to find in Apulia the flexibility and individual idiosyncrasies that one expects in a landscape that was once lived in. Some of these we know to have been original features, but it should also be most interesting to have evidence of how the system fared in its last years, by which time some individuals may well have taken strange liberties with it.

(iii) *Medieval.* In the Medieval landscape, the field-systems of the Roman world have disappeared and there are fundamental changes. We have before us, instead, the traces of a feudal landscape constructed round defended villages and castles. This period has, in many respects, produced the most remarkable results of all, for in this same area of Apulia the plans of about 30 large earthworks have been disclosed by the air photographs.

The majority were visited and examined in 1949, and proved to be the sites of abandoned Medieval villages, motte-and-bailey castles, and Imperial 'hunting-seats' of Frederick II. They also included such important but hitherto unplanned earthworks as those of the deserted Medieval port and town of Salpi, and the Angevin siege-camp against the Saracens in Lucera (probably that of Charles II in 1300), which we have identified for the first time a mile or two outside the town. Many of these sites were altogether unknown, and none of them has been adequately planned or studied on the ground, nor has any pottery been systematically collected from them or excavations made. But with vertical air photographs we have of course the essentials of a site-plan to scale for each one; and we can also often see on the photographs the network of field-systems surrounding them, enclosed by ditches, and the trackways which approached them. On the ground these latter features could never have been appreciated unless the eye was guided to their traces in this way, but *then* they can be readily recognized, either as slight

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[12] The only comparable parallel is the sanded-over centuriation in Southern Tunisia.

[13] See ANTIQUITY, December 1947, 'A Technique for the study of Centuriation', for air-photographs of these.

earthworks or as crop-marks, etc. We devoted some time to the study and identification of the characteristic appearances of these features of the Medieval landscape and to recording them.

Apulia was a ' key-area ' in the early Middle Ages, but though a certain amount is known about the history of its cities the archaeology of all save a few of the field-sites has made very little headway. In the identification of our sites from written sources there do not appear to be any Apulian chronicles that help us very much. But there are a number of charters (**14**), donations, and other documents from the mid-eleventh century onwards which *do* give some information about certain of our abandoned village sites and for Frederick's ' hunting-castles '. I am especially grateful to Miss E. M. Jamison for her experienced guidance in this subject. A discussion of the documents for the latter will be found in the volumes of Eduard Sthamer and Haseloff, *Die Bauten der Hohenstaufen*, but both chiefly concentrated on the most famous and accessible stone-built castles, and lesser-known sites mentioned in the documents remained very shadowy. But their work has provided an indispensable background of historical fact for our field-researches. We can also form some idea of the state of our region in the 14th century from the list of places returning Papal Tenths and their classification as a *casale* (open village), *castrum*, *arcipretura*, etc. (**15**).

Almost all these earthworks which we examined, measured and photographed were extremely rich in surface finds of pottery. Two main types of outstanding importance were evident. The first of these was a class of un-glazed wares, painted with thin red stripes in simple curves and groups of vertical lines on a hard, thin, well-fired, creamy-white ware. Both the ware and its decoration are distinctive and come from sites with occupation proved by documentary sources to extend as far back as the mid-eleventh century (as at San Lorenzo). It resembles a derivative form of the ' Pingsdorf ' ware that was in use in Normandy, and North, Central, and South-west France in the 11th and 12th centuries ; thus, here we may well have for the first time the pottery of the period of the Norman conquerors and settlers of Apulia (**16**).

The other outstanding group consists of glazed polychrome wares, chiefly large dishes (with foot-ring base), plates and bowls, painted principally in green, yellow, brown and (more rarely) shades of grey and blue outlined in brown on a white ground. The composition of the glaze is a matter for analysis. The colour of the biscuit ranges from cream and light buff, to a dark pinkish-buff. At the centre of these vessels are painted spirited animals, birds, fish and human figures, usually singly, while geometrical and curvilinear patterns are very common. The only sherd with incised *sgraffiato* decoration characteristic of the Eastern Mediterranean comes appropriately from the port of Salpi, at which we also found a sherd of brilliant blue Rakka-type ware. These Apulian discoveries have an undoubted relationship with the Levant, and we have very close parallels with the early 13th century material from the American excavations at Corinth (**17**), and from the Crusaders' Castle at 'Athlīt in Palestine (**18**). There is no

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[14] See Caraballese, *Il Comune pugliese durante la Monarchia Normanno-Sveva* and *L'Apulia ed il suo Comune nell'alto medio evo* (vol. 15 and 7, *Documenti e Monografie della Deputazione di Storia Patria per la Puglia*, Laterza, Bari).

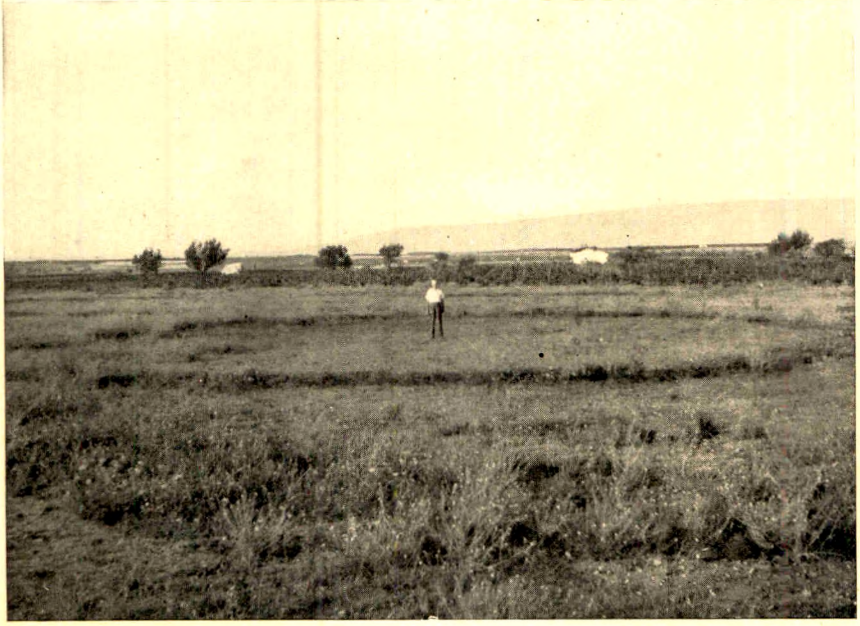
[15] See Domenico Vendola, *Studi e Testi*, vol. 84. *Rationes Decimarum Italiae.*

[16] I should like to thank Mr G. C. Dunning and Mr Martyn Jope for advice in this matter.

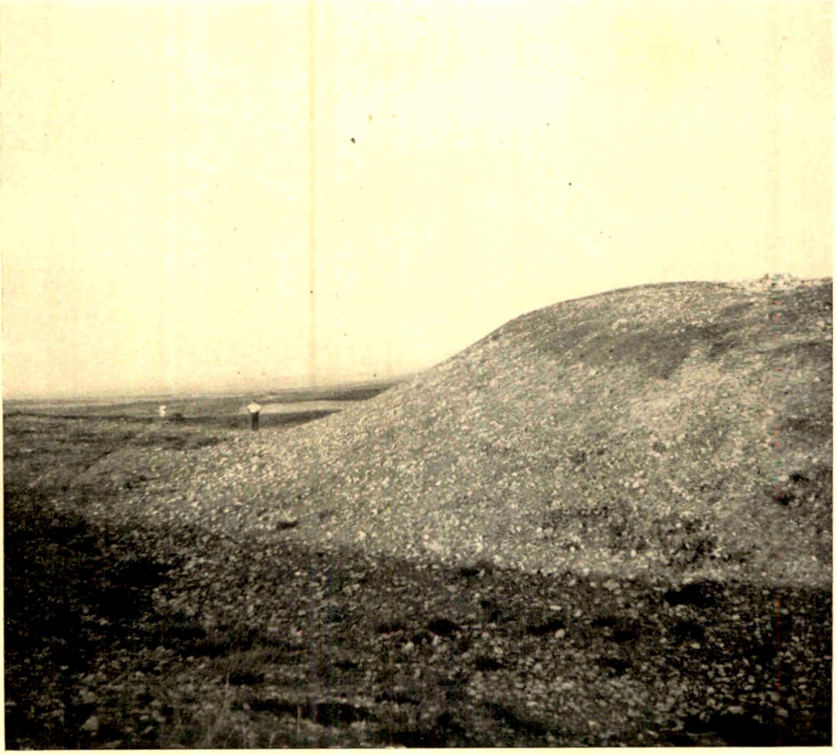
[17] C. H. Morgan, *Corinth*, vol. XI, *The Byzantine Pottery*. Cambridge, Mass., 1942.

[18] C. N. John, *Quarterly of the Dept. of Antiquities in Palestine*, vol. III, 1934, 137 *seq.*, and vol. V, 31 *seq.* On the other hand, parallels from Cyprus are vaguely general rather than particular ; see du Plat Taylor and Megaw, *Report of the Dept. of Antiquities, Cyprus*, 1937–39 (publ. 1949) ' Cypriot Glazed Medieval Pottery '.

PLATE V



A

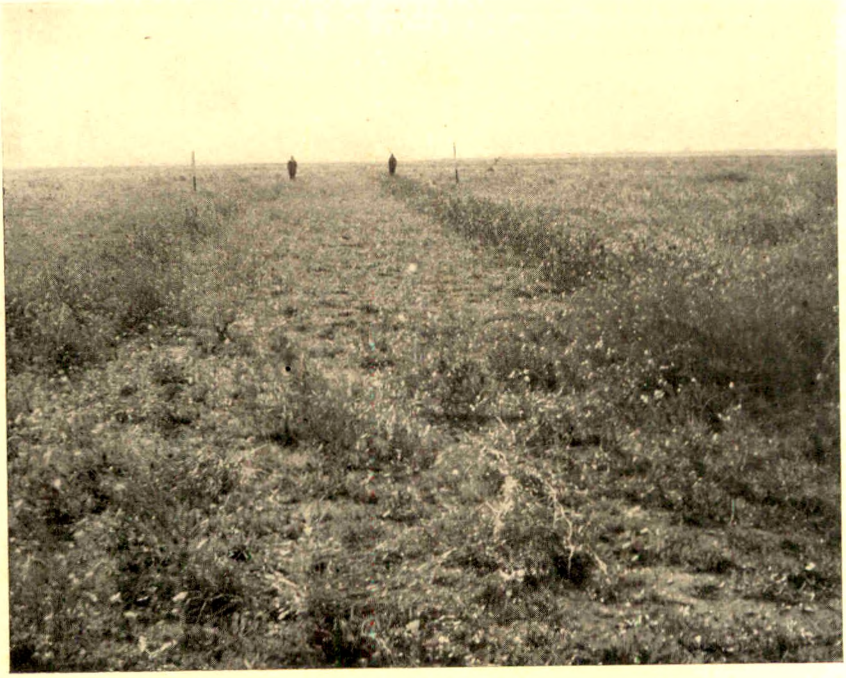


B

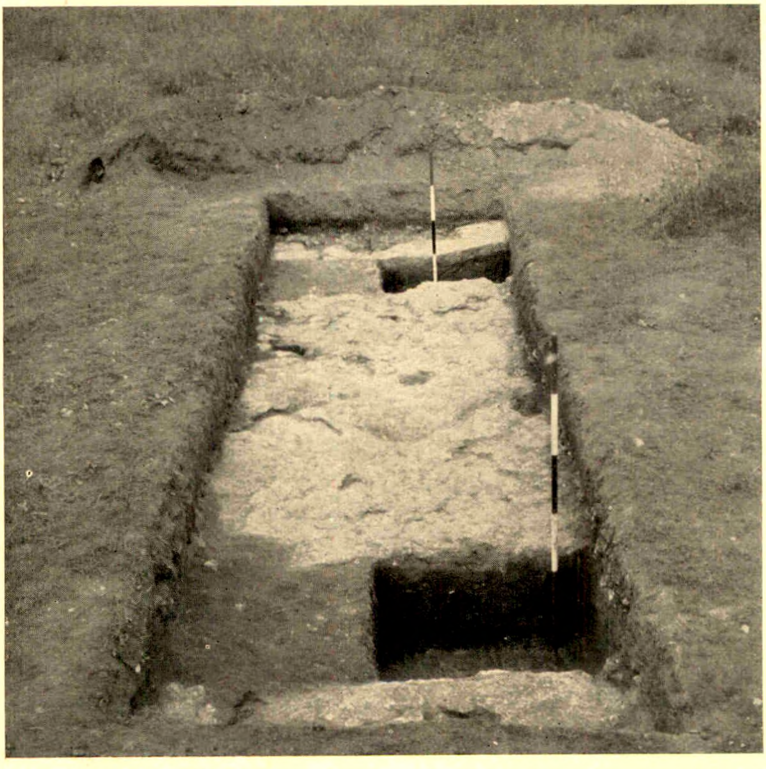
A. A TYPICAL CIRCULAR NEOLITHIC ENCLOSURE IN A SITE OF 'VILLAGE' TYPE, WITH THE POSITION OF ITS DITCH OUTLINED BY A CHARACTERISTIC EXTRA GROWTH OF GRASSES AND WILD FLOWERING PLANTS.

B. CASTELLACCIO. PART OF THE ARTIFICIAL MOTTE, 30 FEET HIGH, OF THIS NEWLY DISCOVERED MOTTE-AND-BAILEY CASTLE. THE DISTANT FIGURES STAND ON THE RAMPART OF THE BAILEY.

PLATE VI



A



B

A. LOOKING DOWN ONE OF THE CENTURIATED ROMAN ROADS IN THE SYSTEM BELONGING TO *LUCERIA*. THE POSITION OF THE DITCH FORMERLY FLANKING EACH SIDE IS REVEALED BY A BUSHY GROWTH OF FLOWERING 'COW PARSLEY'. ROMAN FARM BUILDINGS, VINEYARDS, ETC., WERE SITED ALL ALONG THE ROAD.

B. A SECTION ACROSS THE SAME ROAD, SHOWING THE FLANKING DITCHES (DURING doubt that Apulian polychrome wares had taken a firm hold during the first half of the 13th century. In this relationship between the Eastern and Central Mediterranean, Sicily probably played an important part. The links with the Levant which some of the Apulian polychrome ware reveals must be fitted into the stream of events which were at that time moving on a European scale. We have to remember the often recurring connexion between Apulia and Greece and Byzantium in various forms, in archaeology and history. At the period when we believe polychrome ware first appeared in our region of Apulia, the 40 years from 1189–1229 witnessed four Crusades to the East and included the capture of Constantinople in 1204 (19). There was incessant Crusader traffic and corresponding trade between West and East: the climax of large-scale movements of Medieval nobility across Europe, who possessed the powers of patronage that spread art styles. In the provincial towns of the Eastern Mediterranean they must certainly have seen plenty of local versions of Byzantine polychrome wares to admire and to inspire them with new ideas. It is possible that the origins of Apulian polychrome wares slightly antedate the contacts with the Near East developed through Frederick II's 'orientalizing' (20) outlook, but he was probably the first to foster it there on a large scale. We may note here the fairly rapid passage of polychrome wares, with interesting successive modifications, across Europe in the 13th century via Southwest France to England, even reaching the shores of Sweden. The many problems connected with the origins of Italian polychrome pottery have been much debated. Here in Apulia, at last, stratified and datable series can be excavated from contemporary earthworks. We hope that test-excavations this year will provide this evidence for both the early red-painted and the polychrome types.

Field-research on these earthworks, based on the correlation of ground-air observation, has already supplied enough detail for a book. In the absence here of the necessary apparatus of photographs and plans, one cannot do more than outline their significance. For this purpose I have selected outstanding examples of varying character. Of the new motte-and-bailey castles, the most interesting is that near the farm of Castellaccio, about 5 miles North-east of Troia, magnificently poised on the edge of an escarpment at the back of the plain. The great motte thrown up, surrounded by its ditch, is an impressive affair 30 feet in height (see PLATE V, B), standing in a corner of the bailey which is divided into an upper and a lower ward. The site as a whole is oval and is as much as 330 yards long. It has all the character of a 'fighting-castle', quite unlike the residential 'hunting castles' on the plain. Its documentary record is singularly obscure. The whole site is thickly strewn with the un-glazed red-painted wares already discussed and thought to resemble Norman 'Pingsdorf' pottery. This site has been selected for test-excavation to obtain a stratified series to confirm this if possible.

Pottery of the same kind was also found by us inside San Lorenzo (21) in a newly

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[19] Similarities with finds from Byzantium itself are generic though sometimes rather faint. The crude but lively 'white painted ware' illustrated by D. Talbot Rice, *Byzantine Glazed Pottery*, plate XII, assigned to the 13th or 14th centuries, is analogous but simpler; for stratified, and datable, material see Martiny, Stevenson and Brett, *The Great Palace of the Byzantine Emperors*, 1947, but it does not seem to provide the prototypes for the Apulian developments.

[20] That such close links existed ceramically at Lucera can be seen in pottery of this period from the Castle, especially in those jugs with filters inside the neck which also occur at Fostat, the site of earlier Cairo, and these must be a direct import (cp. Arthur Lane, *Early Islamic Pottery*, 1947, plate 36, D, E). They are in Lucera Museum.

[21] For vertical air-photograph showing its plan, see ANTIQUITY, June 1949, plate VI.

excavated vineyard, and we have references to the site fom the mid-eleventh century onwards. The archaeology of this huge and complex earthwork, 3½ miles south of Foggia, must await our full report, but now as an essential preliminary we already have the first accurate plan of its triple enclosures, 800 yards overall, with earth ramparts up to 25 feet high. An interesting feature was the surrounding system of field-drainage canals, essential in this marshy setting, which are referred to in the early medieval documents. Here as on so many of our deserted medieval sites we found sherds of Roman Arretine. Some of our finest polychrome sherds came from within this site, including one showing a full-length robed figure closely paralleled on a bowl from 'Athlīt. This might well be expected from a site that was one of Frederick II's favourite ' hunting-castles ' at which we know that he frequently stayed. But if San Lorenzo is scarcely known, the very existence of the great earthwork at the farm called Petrulla (midway between Foggia and Manfredonia) had been completely forgotten. The site appears to be that of the medieval San Chirico (22) which lay hereabouts, though it is an adjacent farm that actually bears the name today. Its size and defences are impressive. There is a rectangular outer ward *c.* 470 yards long enclosed by an earthen rampart up to 15 feet high, with an inner ward *c.* 290 yards across inserted into one side. The slopes of the rampart of the inner ward produced much very fine 13th century polychrome pottery.

We may take the site today called Torrione del Casone between Foggia and San Severo as an example of the hitherto unidentified Medieval villages (23). Probably it fell on evil days and was abandoned like other sites in this region in the late Middle Ages, when the Kingdom of Naples attempted to monopolize the whole area for sheep-farming, bringing ruinous conditions which prevailed until very recent times. The site is roughly square, measuring 430 yards across, with a strong earth rampart standing 10 feet high. Inside, two axial streets (identified from the air) can be seen as broad ' hollow ways ' coming in from the entrances and crossing at right-angles, with building-mounds along them (one being 30 paces across), The whole is dominated by the remains of a lofty stone tower within a deep circular ditch, at the centre of the site. Polychrome wares of 13th–14th century type are abundant all over the surface, and at one point we found undoubted evidence of a kiln, with glazed ' wasters ' and a large natural pebble experimentally dipped in an opaque greenish-white glaze. This is a fact of importance, for we also found glazed kiln-waste at Salpi, indicating local centres for the manufacture of glazed wares. The excavation of such a kiln might well solve many problems.

Perhaps the most spectacular site, and one of which scarcely anything was previously known, is the Medieval port of Salpi whose detailed plan the air photographs provide. Of the Roman *colonia* here hardly more than the name is known, but documents tell us of an early Medieval village, a *domus* of Frederick II, and a port still sufficiently active for the Venetians (24) to covet its harbour and to seek to obtain this and Barletta when engineering an agreement with the Papacy against Frederick in 1239. Today it is a wild and remote spot, on the edge of a great desolate lagoon converted to salt-pans. A massive earthen rampart, often 25 feet in height, encloses a sub-rectangular area *c.* 340 yards square, with two entrances on the North and two on the South approached by ancient trackways, which were identified from the air and on the ground. Within lies a tall and large isolated motte, and on the side towards the lagoon are two important

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[22] It is mentioned in documents of the Norman period and still ranked as an *arcipretura* or *pieve*, liable for Papal Tenths in the early 14th century.

[23] See air photograph in ANTIQUITY, June 1949, plate vb.

[24] Cp. Kantorowicz, *Frederick the Second*, 1930, p. 476.

ditched enclosures set against the rampart. The commanding views in all directions extended as far as Castel del Monte. The interior and the broad ditch were carpeted with medieval sherds so thickly that one could sweep them up with a dustpan and brush! We shall return for further work on this site.

These discoveries have a major importance intrinsically, from their nature and numbers, in the study of each of the three main periods under review. But, when considered collectively as successive episodes, the sum total is very much more than three separate investigations. Apulia lies at the heart of the Mediterranean world, and a thorough study of the organic changes in the cultural landscape of a restricted area in it, with which one can really come to grips closely, is a new and promising method to apply. Only such a factual and objective basis could earn the right to formulate generalizations and to build up a synoptic view. The results of this research should help to clarify the evolutionary perspective behind a conservative and traditionally-minded peasantry, representative of many similar regional groups in Southern Europe. Had we concentrated on one period alone this would have been impossible, and we intend to exploit to the full for the advantage of the Humanities these fruitful gifts of scientific technique. Implicit in all the detailed work that has gone on, and is yet to be done, lies this central idea of the study by archaeological means of a Mediterranean peasantry at three vital stages in its growth. Archaeology is after all more than a collection of specialist studies. By treating our theme thus, we may come a little nearer to the roots of European civilization.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

DIE GOLDKANNE VON ST. MAURICE D'AGAUNE, by A. ALFOLDI. 1 colour-plate, 18 plates, 27 pages. *Zeitschr. für Schweizerische Archaeologie und Kunstgeschichte*, Band 10, 1948. [The splendid enamels of this jug are interpreted as having formed part of a sceptre of the Avar Khans, looted by Charlemagne and given to St. Maurice].

GIRALDUS CAMBRENSIS IN TOPOGRAPHIA HIBERNIE, ed. by JOHN J. O'MEARA: text of the first recension. *Proc. R. Irish Academy* LII, C. No. 4, Dublin, 1949. [A trustworthy version of the original Latin text of a valuable source of early Irish history].

RECHERCHES SUR LA CULTE DES RELIQUES ET L'ART CHRÉTIEN ANTIQUE, by ANDRÉ GRABAR. Two vols. 8vo: *I, Architecture*, 638 pages, 135 figs. *II, Iconographie*, 409 pages, 12 figs.: also Album of 70 plates. Collège de France, Paris, 1946. [Reviewed in *Journal des Savants*, July-Dec. 1948. This important work appears to have been overlooked in this country].

THE DEAD SEA SCROLLS, by G. LANKESTER HARDING. *Palestine Exploration Quarterly*, July-Oct. 1949, 112–16. [Severe criticisms of procedure.]

REPORT ON EXCAVATIONS ON THE CAMBRIDGESHIRE CAR DYKE, 1947, by J. G. D. CLARKE, F.S.A. [The Car Dyke proved by these excavations to be a Roman canal constructed in the 1st cent. A.D., remaining in use 'till towards the close of the 2nd century', but not used 'during the later stages of the Roman occupation'. It was probably made originally to meet a strategical need, i.e. transport corn to the troops in the north].

Notes and News

RHODESIAN CULTIVATION TERRACES

The photographs which we publish here show portions of a vast system of terrace cultivation, and the homesteads of the cultivators, near Inyanga in Southern Rhodesia. By the time that the following remarks appear in print it is probable that excavations will have begun, so that a full account of the remains would be premature. Our object in publishing this note is two-fold: (1) to give readers some general information about a matter of topical interest which may be mentioned in the press; (2) to enlist support for the Inyanga Research Fund, which has been founded under the patronage of the Governor of Southern Rhodesia, who recommends it as 'deserving the strongest support'. We do so too, and would urge those of our readers who wish to help to send their contributions to the secretary of the Fund (Mr Neville Jones), P.O. Box 240, Bulawayo, Southern Rhodesia.

It will be obvious at a glance that the remains consist of thousands of terraces deliberately constructed on the steep slopes of the hills, of roughly circular and oval stone enclosures, and of paths leading between walls to those enclosures (see especially PLATE VII). The terraces are not lynchets—which are formed by the *natural* accumulation of soil on the lower sides of fields—but strips of ground on whose lower side a wall has been built to reduce the steepness of the slope and to hold the soil (PLATE VIII). Only by such measures could the cultivation of such steep slopes be possible. The construction of these terraces must represent an enormous amount of labour, and it is legitimate, even without the evidence of excavation (which alone can be decisive) to conclude that the whole complex must be measured in centuries. But it would be rash to suppose that it must have covered many centuries and still more so that it is very ancient. In fact in a climate of heavy rainfall which is continually eroding the surface any great antiquity is ruled out.

So much may be inferred merely from the evidence here published. The appearance of the enclosures supports these inferences. They resemble those other enclosures which have been proved by excavation to be of medieval or post-medieval date. At Niekerk Dr Randall MacIver excavated similar enclosures, finding hut-platforms and the remains of the wattled walls of which the huts were built 'exactly as the modern Kaffir makes them, viz. of a circle of stakes, strengthened by wattle lashings inside and outside, against which thick clay is plastered, so as generally to hide the wattle-work (1)'. He also found grinding-stones, proving agriculture. His photographs (Plates VI and VII) show lines of terraces exactly like those here published; but he strangely failed to interpret them correctly, regarding them as constructed for defence purposes.

It is natural to connect all these remains with the famous ruins of Zimbabwe The name has been interpreted as meaning 'stone houses', though not all Bantu linguists agree on this point; it is however certain that more than one site bore the name. It seems pretty certain that the famous Zimbabwe was the place called Masapa, the capital of 'Monomotapa' who ruled a large territory in the 16th and 17th centuries and had relations with the Portuguese, whose writers have left contemporary accounts of him.

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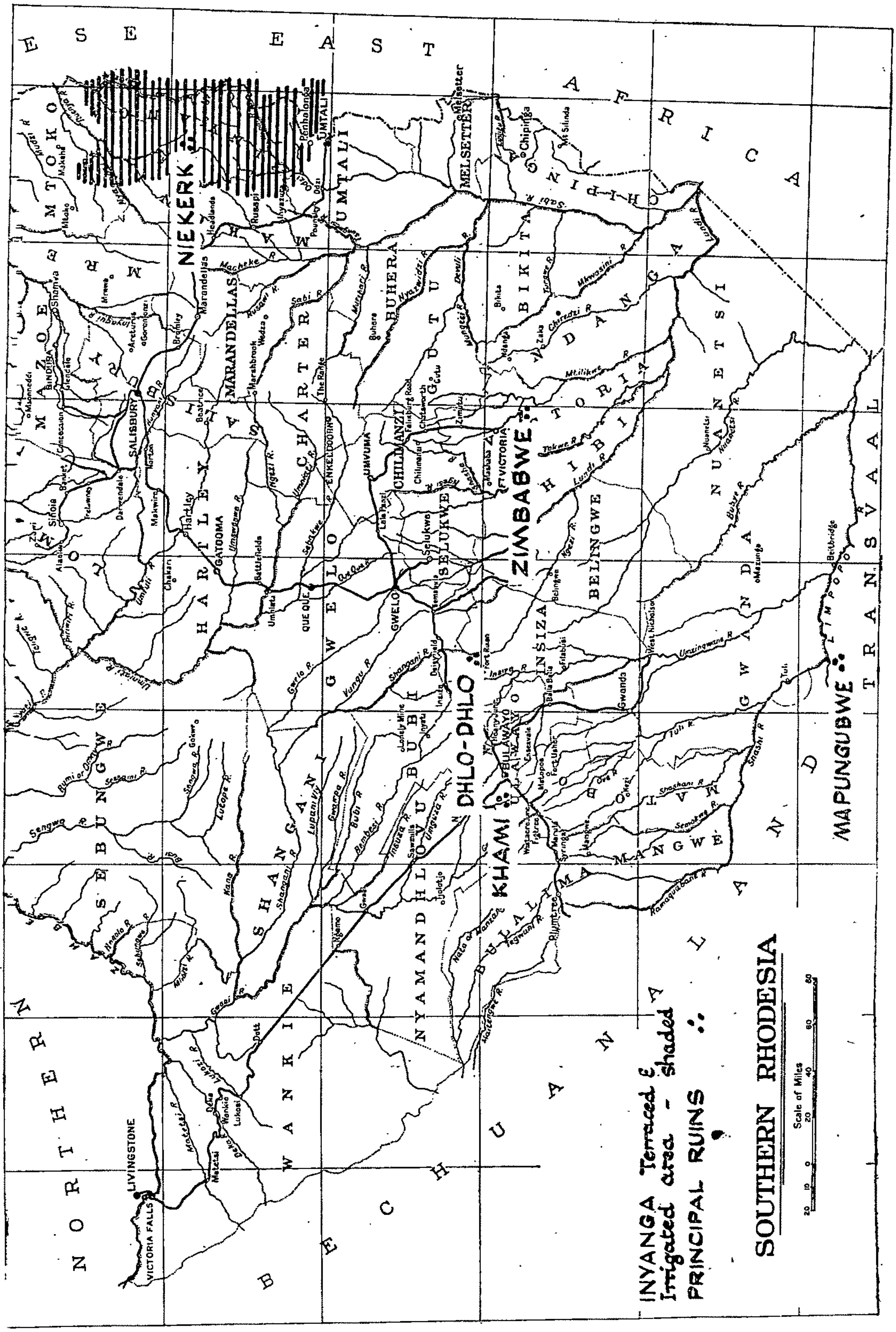
[1] *Medieval Rhodesia*, 27.


SOUTHERN RHODESIA
INYANGA Terraced & Irrigated area - shaded
PRINCIPAL RUINS ⁘
Scale of Miles
20 10 0 20 40 60 80
PORTUGUESE EAST AFRICA
NORTHERN RHODESIA
BECHUANALAND
TRANSVAAL
NIEKERK
ZIMBABWE
DHLO-DHLO
KHAMI
MAPUNGUBWE
UMTALI
MELSETTER
SALISBURY
MARANDELLAS
HARTLEY
GATOOMA
QUE QUE
GWELO
CHILIMANZI
SELUKWE
FT VICTORIA
BULAWAYO
LIVINGSTONE
VICTORIA FALLS
Wankie
Beitbridge
Gwanda
Belingwe
Inyanga
Penhalonga
Chipinga
MTOKO
MAZOE
MRewa
CHARTER
UMTALI
BUHERA
NDANGA
BIKITA
CHIPINGA
VICTORIA
CHIBI
BELINGWE
NUANETSI
GWANDA
MATOBO
BULALIMA MANGWE
NYAMANDHLOVU
INSIZA
BUBI
SHANGANI
WANKIE
SEBUNGWE
LOMAGUNDI
HARTLEY
GWELO
SELUKWE
LIMPOPO R.
Sabi R.
Lundi R.
Tokwe R.
Shangani R.
Gwaai R.
Umniati R.

The name seems more likely to be generic—perhaps meaning ' ruler '—than a personal one, but here again the authorities differ. (A mid 17th century missionary describes him as a ' black man '.) ' There is no difference whatever in essential character between the rough buildings that are found in the Inyanga district and those of the Monomotapan capital near Victoria. Zimbabwe reproduces every feature of the modern sites, only with more elaboration and on a larger scale. The " Elliptical Temple " (2) can be completely explained as a combination of the developed form of Inyanga hill-fort with such a stone-fenced kraal as Nanatali ' (3). A little further on Dr MacIver puts these visible ruins in their correct setting, saying : ' It is . . . the huts which constitute the really essential part of the ruin in every case ; the stone wall which the visitor so much admires is only the skin, the huts are the flesh and bone '.

We would warn our readers that the forthcoming excavations will probably be the signal for an outburst in the press of the usual mystery-mongering and the re-hash of stale speculations about King Solomon's mines and similar fantastic rubbish. Excavation has twice over disproved the great antiquity of the Zimbabwe culture, and has shown good reasons for regarding it as ancestral to the recent African culture of the region. The date of the *beginning* of the Zimbabwe culture has not yet been determined ; but it was certainly flourishing at the beginning of the 16th century and may be a few centuries older—it is unlikely to be more. Earlier dating depends upon imported objects such as beads and Chinese pottery(4) whose dating is mainly typological, and for that reason to be accepted with reserve ; and the latest excavator of Zimbabwe has stated her opinion that the earliest stone structures in Southern Rhodesia ' cannot on any available archaeological evidence be placed as earlier than the 10th century and may be any amount later ; and that the latest cannot, on any available archaeological evidence be placed as earlier than the Ming period, 12th century, and may be—and almost certainly are—as late as the 16th century '.

One last point :—Excavators of these ancient sites constantly, and rightly, call attention to features in them which resemble those of the modern local African culture. One such has been mentioned above. But is there any easily accessible publication describing that culture ? Does there exist an accurate plan based on survey of a typical village, a large scale plan, with sections and detailed description of structure, of a typical dwelling, or a detailed, illustrated account of the internal economy of such a community ? The word ' detailed ' is the operative one ; generalities abound, but are useless for comparative purposes. The description of the hut should say, for instance, how its walls are made and whether it stands (as do the ancient ones) on a platform. All these things are of course common and obvious to-day—or were yesterday, when MacIver wrote. They may already be gone ; they will certainly not survive much longer. Here is a piece of work for an anthropologist ; one whose value will increase as the years go by. Further excavators of Rhodesian ruins will not be able to compare them with modern African culture, as recent excavators have done ; they will be entirely dependent upon such descriptions, plans and photographs as may be produced to-day and to-morrow, but not for long after that.

O.G.S.C.

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[2] Which of course is not a temple at all.

[3] *Medieval Rhodesia*, 84.

[4] A Chinese bowl of Ming ware was found at Dhlo-dhlo by Miss Caton Thompson but expert opinion on its date differs by five centuries : see ANTIQUITY III, 1929, 432–3.

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E. M. Finch. ‘Pit People of the Inyanga Downs’. *Proc. Rhodesian Scientific Association* XLII, (1949), 38–58.

AGE DETERMINATION BY RADIOCARBON CONTENT

A very promising method for determining the age of certain types of archaeological and geological material has been and is under development by Professor W. F. Libby of the Institute of Nuclear Studies of the University of Chicago, Chicago, Illinois, U.S.A. The details of this method, recently summarized by Dr Libby and his Colleagues, Drs E. C. Anderson and R. J. Arnold,* are highly technical and beyond the comprehension of those who are not specialists. In brief, it depends on the fact that all living matter has a uniform radioactivity associated with its carbon content. Assuming this to have been true in times past, and that at death all further supply of radiocarbon (C^{14}) ceases, one may then hope to calculate the time elapsed since death by a count of the residual radioactivity of the isotope Carbon^{14} in the organic specimen. For example, the present assay is 12.5 Carbon 14 explosions per minute per gram of carbon; it is therefore expected that an ancient sample 5700 years old (the half life of Carbon^{14}) would show 6.25. This has proved to be true in a number of samples of known date, which Dr Libby and his associates have already measured, and at present they are engaged in attacking several additional dating problems, in order to obtain further checks on the method through internal consistency comparisons. This work is going steadily forward, and it is understood that since the beginning of 1949, when the paper quoted above was written, a great deal of progress has been made.†

A Committee has been created by the American Anthropological Association and the Geological Society of America to collaborate with Dr Libby and his associates, to develop ideas concerning general policy, and to clarify the procedure and the objectives of the programme. This Committee felt, however, that it was not in a position to secure the desired samples, and for this reason a number of archaeologists and geologists were requested to join the programme as Collaborators. To represent the Old World, the Committee accordingly has appointed Dr R. J. Braidwood of the Oriental Institute, University of Chicago, as Collaborator for the Near and Middle Eastern areas, and the writer as Collaborator on Upper Palaeolithic, Mesolithic, Neolithic, and later periods in Europe, Asia, and Africa, exclusive of the Near and Middle East. Pertinent material for Dr Libby and his associates, therefore, should be sent to me at the Peabody Museum of Harvard University, Cambridge 38, Massachusetts, U.S.A., since, in order to develop this method of dating, it is necessary to obtain as many completely documented samples as possible from known archaeological and geological horizons. The samples which are desired should come from localities, the estimated age of which is more than 2,000 years and less than 25,000 years. In each case these samples should be large enough to allow several determinations of age on each, in order to permit checking the method, reducing the error, and thereby perfecting the process. Before submitting material to the writer, however, from Upper Palaeolithic and later horizons in the Old World, exclusive of the Near and Middle East, close attention should be paid to the following critical points :

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* See *Science*, vol. 109, no. 2827 (March, 1949), pp. 227–8; also ‘Editorial Notes’ in ANTIQUITY, vol. 23, no. 91 (September, 1949), pp. 113–14.

† Compare paper by Drs Arnold and Libby on ‘Age Determinations by Radiocarbon Content: Checks with Samples of Known Age’ (*Science*, vol. 110, no. 2869 [December 1949], pp. 678-80); also summary of Dr Libby's recent lecture in London, by I. W. Cornwall, *Archaeological News-Letter* [April 1950], pp. 177-8).

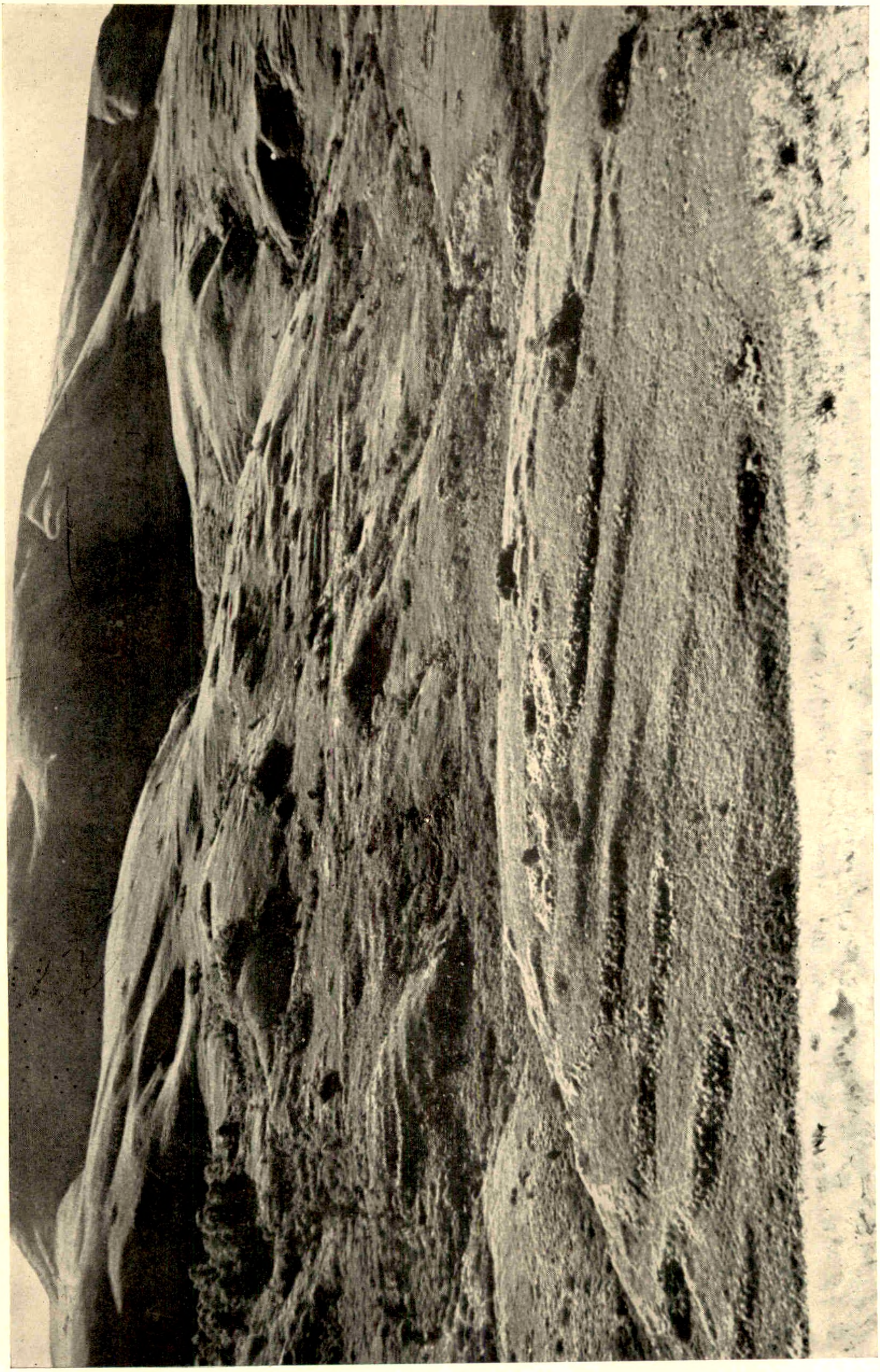
1. Since Dr Libby and his associates, as well as the Committee, are interested in all parts of the world, it is anticipated there will be such great quantities of specimens submitted that it will be necessary to assess priorities. Therefore, a letter should be sent with each sample(s), clearly covering the following details :
   - (*a*) The specimen should contribute toward the solution of a real chronological problem. Since at the moment the method cannot be more accurate than ± 200 years, it is obviously not applicable to the solution of detailed historical problems. Its application is pertinent to prehistoric, protohistoric, and the earliest historic ranges only.
   - (*b*) The specimen should have a very positive archaeological or geological context or association, so that the date obtained from it will have significance.
   - (*c*) The specimen should *not* have been treated or preserved with a chemical solution. If it has been, and it is definitely known what chemicals were used, the specialists may be able to reclaim the specimen.
   - (*d*) If there were any peculiar circumstances concerning the stratum in which the specimen was found, this should be carefully explained in the letter.
2. The materials which constitute acceptable specimens (*all specimens are destroyed in the process of analysis and cannot be returned under any circumstances*), in the requisite amounts, are as follows :
   - (*a*) 65 grams of charcoal, *but* 200 grams of all other types of vegetable remains—wood, peat, grain, straw, grass or reeds, matting, basket or cloth fragments, etc. Although it is anticipated that a method will be developed for treating ash from hearths and charcoal bits from any darkened floor debris, none has yet been determined.
   - (*b*) 200 grams of epidermal animal remains—skin, hide, hair, nails, claws, or horn—but of antler it is necessary to have 500 grams.
   - (*c*) 2.2 kilos (5 pounds) of teeth, ivory, or charred bone.
   - (*d*) 700 grams (1½ pounds) of shell.

   The quantities noted here assume that the specimen is reasonably dry and free from excess dirt. In cases where only lesser amounts are available from strata which have great chronological importance, Dr Libby and his associates may even be able to obtain results with less than half the above amounts, but a sufficient quantity for more than one counting is desirable. It is also advantageous for checking purposes

PLATE VII

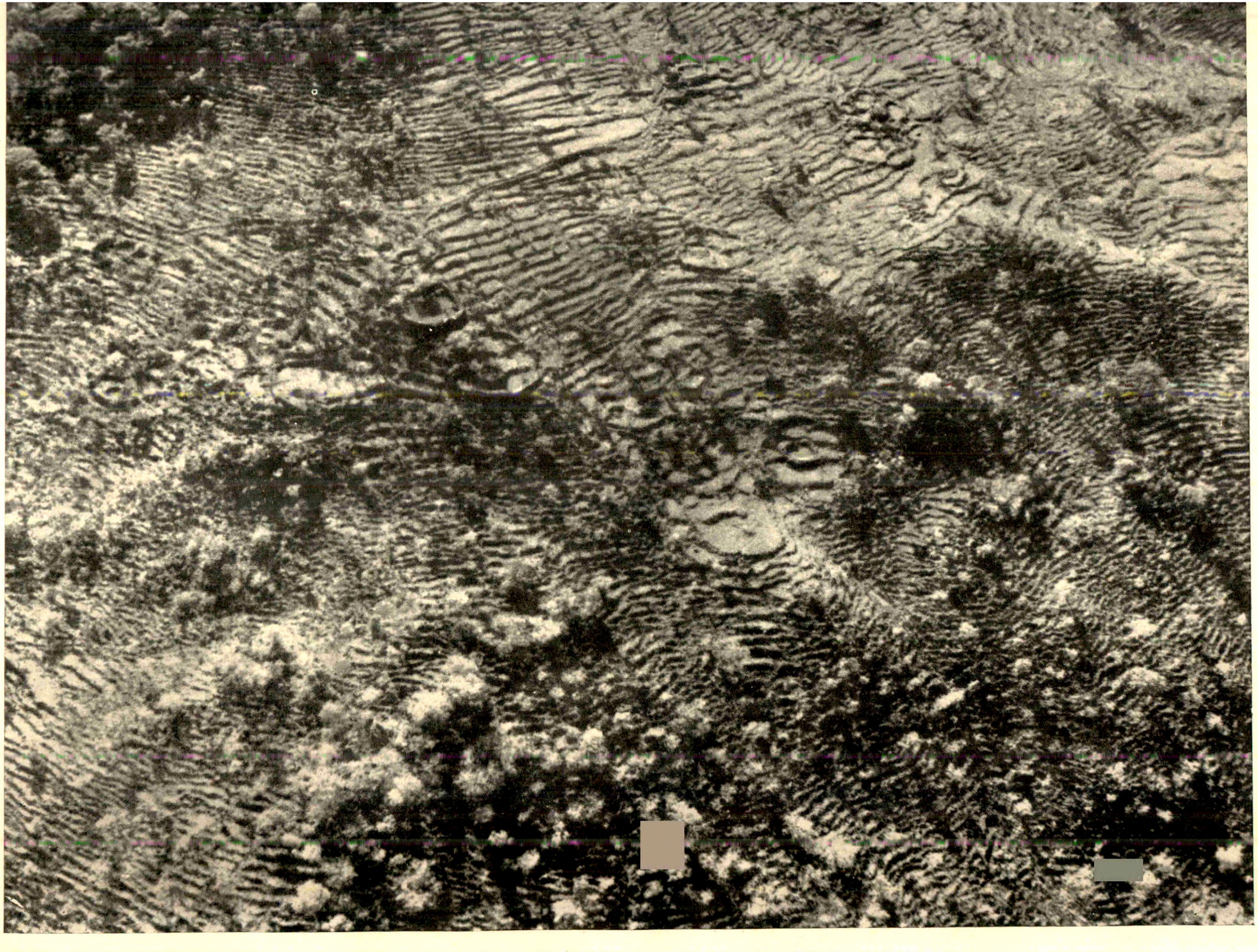
facing p. 100

PLATE VIII

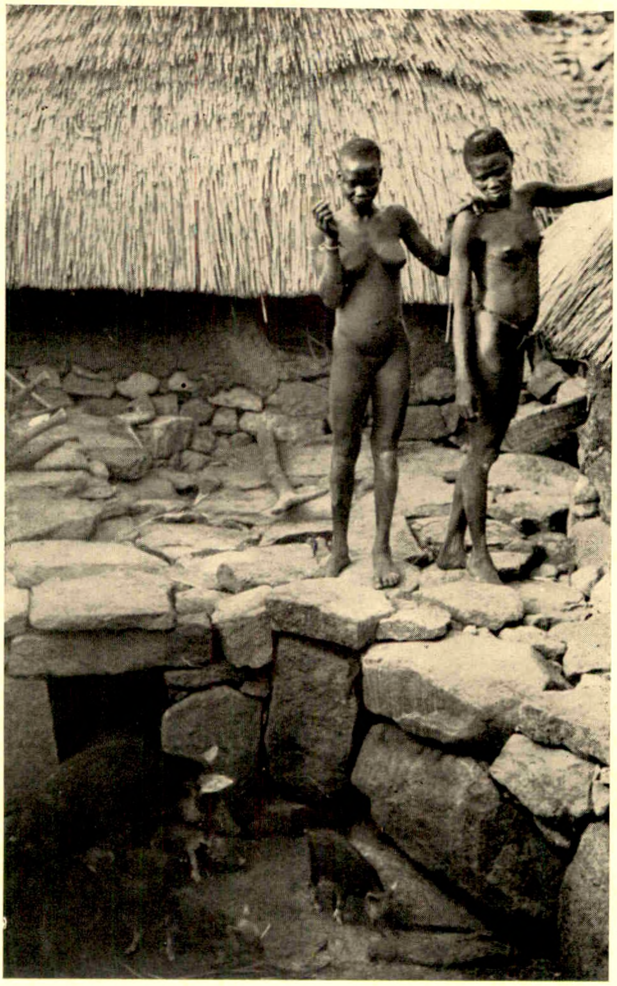


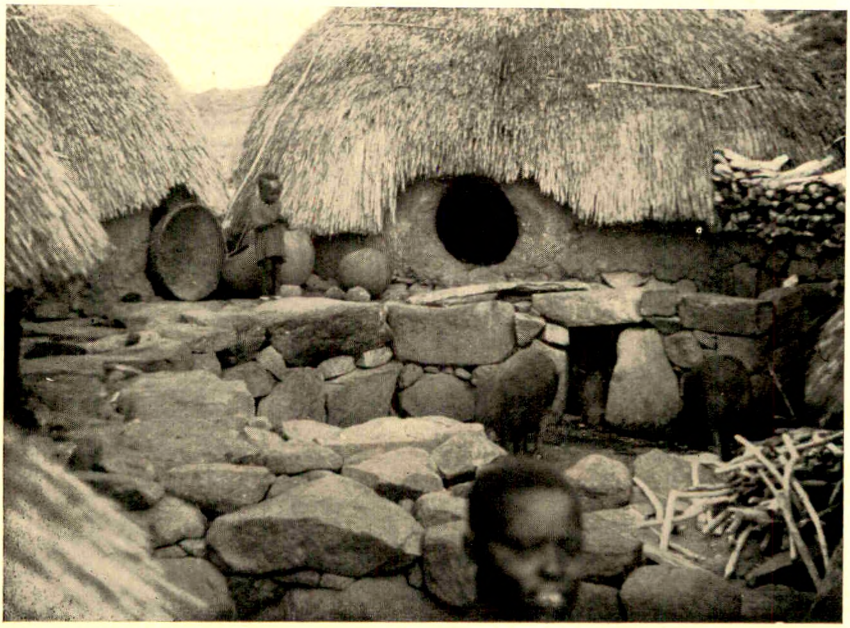
GROUND-VIEW OF CULTIVATION-TERRACES NEAR INYANGA, S. RHODESIA
Note remains of rough stone buildings on top of low hill in foreground.
Copyright photograph by E. T. Brown, Umtali

PLATE IX



TERRACE AND STONE KRAALS, INYANGA, S. RHODESIA





PIGSTY IN NUBA MOUNTAINS, SUDAN

if specimens in more than one of the categories of materials are available. It should be noted that *unburned bone* is *not* listed, since it does not seem promising.

3 Specimens should be separately labeled and wrapped ; i.e. placed in a strong corked bottle, or wrapped in tin-foil or tissue paper, or some other easily separable wrapping—*not* simply enveloped in straw, excelsior, or wood-shavings—and carefully packed in a wooden or tin box for reshipment. In order to facilitate clearance through the U.S. Customs and Postal authorities, all packages should be inscribed 'Scientific Specimen: No Commercial Value'.

It is realized that specimens may not be available at the moment. In such cases, this announcement will serve as a guide in collecting samples in the future. It is hoped that no potential specimens will be lost, and that workers who may chance to visit old sites will watch carefully for pieces of charcoal or other organic materials, which can be obtained from *known* levels exposed on the walls of the trenches of old excavations.

The writer will try to have those specimens which are submitted to him processed, if they fit into the general programme suggested above, as soon as possible after their arrival, and to report to the sender as soon as Dr Libby and his associates have released the results of their test. He also agrees to treat the contents of this report as confidential. In this connexion, it should be borne in mind that considerable control over the announcement of dates is being exercised by Dr Libby and his associates, the Committee, and the various Collaborators. Since these are the initial results of an as yet incompletely known method, it is felt that the premature announcement of a single date, or an incomplete series of dates, without adequate comment, will only lead to misunderstandings, speculations concerning their significance, and conclusions which may prove to be quite untenable. For this reason, Dr Libby and his associates are the sole judges regarding when dates are ready for release. At the proper time they will inform the Collaborators of their results, and a report will be prepared on that particular project shortly after this. Institutions and individuals supplying samples for analysis will be informed of the results on their samples as soon as the report is in an advanced stage, and when the significance of the dates is clear.

In short, the radiocarbon programme is still in its infancy. Nevertheless, it gives such definite promise of striking at the shackles of imprecise chronology in the fields of pre- and proto-historic archaeology, and Late Glacial, as well as Early Post-Glacial, geology that it is certainly deserving of all the support and cooperation which workers in these fields in the Old World can give it.

HALLAM L. MOVIUS, JR.

THE PIGSTY HOUSE IN THE NUBA MOUNTAINS

Encouraged by Professor Stuart Piggott's plea for present-day examples of primitive houses made at last year's Prehistoric Society conference on prehistoric houses and farms, I send two photographs of a typical Moro homestead in the Nuba Mountains, Anglo-Egyptian Sudan. There the pig is housed in a permanent stone construction, consisting of low stone 'rooms' with rectangular entrances and flat roofs, surrounding a roughly circular open space in which he can take exercise and enjoy the sunlight, without fear of escape. His master lives with his family in four or five circular mudhuts with round doorways and thatched roofs, which are built around the pig-run on the stone substructure in which the pigs live. In a climate such as that of the Nuba mountains where torrential rain falls during the rainy season, if a homestead is deserted the human habitation disappears in a few years, leaving the stone pigsty containing a few sherds and other relics of human occupation from above to puzzle the archaeologist of the future as to how the pigsty can have housed a race of pigmies.

A. J. ARKELL.

A NEW TECHNIQUE FOR REMOVING BONES FROM LIMESTONE BRECCIA

Osseous remains embedded in breccia are not uncommonly found during archaeological excavations practised in limestone regions, specifically in areas surrounding the Mediterranean. Such remains, some of them human, were discovered in large numbers during the Palaeolithic excavations of the Boston College—Fordham University Expedition at Ksar ‘Akil, near Beirut, the Republic of Lebanon (1). The breccia was found close to the wall of the rock shelter ; at other sites, the removal of less compact materials by various agencies has given rise to the phenomenon known as ‘ hanging breccia (2) ’. The thickness of the breccia at Ksar ’Akil averaged some fifty or so centimetres from front to back. In rock shelters, unfortunately, it is formed in the most protected portion of the deposits, hence in the area which was most desirable for human habitation and now most fruitful in artifacts and bones. Because of the nature of this substance, one recoils from using the primitive methods hitherto employed in working it, i.e., removing it in chunks with drills and then breaking it up with hammers and chisels. Even when a human skeleton has been removed in a block and transported to much more comfortable working quarters in a laboratory, the amount of mechanical work needed to free the bones is appalling.

The breccia of which I speak is composed of soil, limestone chips, flint tools and bones. The soil is derived from disintegrated limestone, and therefore contains considerable amounts of iron compounds and lesser quantities of those of manganese. The agents which cement the miscellaneous components of breccia into a secondary rock are predominantly calcium carbonate (from the limestone) and, to a very small degree, phosphates (presumably from the bones).

The bones occurring in such a breccia are marked by two difficult characteristics (apart from their perverse tendency to break in the wrong direction) : they are more fragile than the surrounding matrix, and they contain more calcium carbonate than ordinary bone. The importance of this fact will appear in a moment. The bones do not provide planes of cleavage, except on rare occasions, hence percussion is a risky process. In addition, it often happens that a rather delicate bone has been crushed, fragments of the bone being intermingled with the matrix in such a manner that purely mechanical treatment would be a matter of seemingly endless patience and time.

Being desirious of extricating some human remains from such a breccia with the minimum amount of damage to the specimens and with the expenditure of a minimum amount of time, the writer set himself the problem of developing a chemical method of freeing the bones from the matrix (3). The report on the results of my investigations falls into four categories : (1) The use of acid ; (2) methods of hardening delicate bone ; (3) the protection of exposed bone ; (4) the optimum conditions for the employment of the method devised.

(1) It would seem that the main constitutent of bone, after the organic matter and the calcium carbonate have been removed, is α tricalcium phosphate. The same may be said for the dentine and cement of the teeth ; the enamel, however, is approximately 60 per cent carbonate apatite and 30 per cent α tricalcium phosphate, the remainder being calcium carbonate and other mineral and organic matters (4). However that

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[1] Ewing, 1947 ; 1949.

[2] Garrod, 1937, p. 123.

[3] I must express my deep gratitude for assistance during the early stages of this research to Professor Douglas Hennessy, of the Department of Chemistry, Fordham University, N.Y.

[4] Dallemagne, 1946, *a* and *b*.

may be, it was clear, even before analysis, that the particular bones with which I wished to experiment contained far more than the normally small amount of calcium carbonate, because of their saturation by lime-bearing water from the country limestone for thousands of years. It therefore behoved me to proceed with caution in applying any acids.

It has, of course, long been known that acids will attack bone; the microscopists have used phosphoric, hydrochloric, acetic, lactic and other acids for decalcifying bone (5). The use of acid against stone possesses a very respectable antiquity, as witness the chemical experiment of Hannibal while crossing the Alps (6). Acids have also been used in various ways by palaeontological preparators in isolating fossilized specimens (7).

A large number of acids (8) were applied to specimens of animal bone taken from several layers at Ksar 'Akil, and the conclusion was finally reached that weak acetic acid, partially buffered with sodium hydroxide, gave the best results. The amounts found most favourable were:

Glacial acetic acid	60 cc., dissolved in
Water	1 litre; to this
Saturated Sodium Hydroxide solution ..	10 cc. is added.

Other conditions besides the kind and concentration of acids were varied. The effect of the cold solutions was compared with that of solutions heated over a steam bath. The use of heat is not recommended. Several wetting agents (9) were tried, but are evidently not worth the trouble, in terms of weight of matrix lost during a definite time.

The formula finally settled upon is effective, but it has its limitations. Its action upon the matrix is slow, and hence large masses of breccia would have to remain in the solution for a long time; on the other hand, it was found that unprotected bone should not be allowed to be exposed to the acid for more than three or four hours, lest the bone too be attacked and eroded. This is especially true of exposed cancellous parts of the bone; the outer table resists erosion for a longer time.

(2) Therefore I was led to consider the possibilities of hardening the bone and thus protecting it from the action of the acid. Very often, as in the case of the walls of the maxillary sinuses or of cancellous tissue, the bone needs to be hardened before it can be handled at all. Various experiments were made with substances which might be expected to harden the bone during the acid treatment (10), but no such substance was found satisfactory. The best means of hardening bone known to me is impregnation with

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[5] Gatenby, 1937, pp. 250–1; 430–1.

[6] Livy, Bk. XXI, 37.

[7] Camp, 1937, pp. 38–9.

[8] I will mention reagents which proved less adapted to the problem, as thus I may save other investigators time and energy. Particularly to be avoided are phosphoric acid, the action of which soon slows down to a stop and the products of which seem to prevent any other acid from working well; and citric acid, which in any concentration attacks the bone vigorously. Dallemagne and Mélon (1946*b*) used citric acid to remove the free calcium carbonate from bone; the notable and destructive action of this acid on my specimens was a good indication of the presence of excess calcium carbonate.

[9] Especially Cetab and Rodalon.

[10] e.g., various phosphates, sodium hexametaphosphate, etc.

Alvar (11). This will, indeed, temporarily retard the etching action of the acid, but it does not offer permanent protection to the bone. After cleaning and thorough drying (air drying is preferable as against drying in an oven), Alvar should by all means be used (12).

(3) It became evident that the intelligent thing to do would be so to protect any exposed bone that one might safely use not only the weak acid solution for long periods in order to reduce the matrix, but also high concentrations of acid, which would accelerate the process. When one approximated the bone, one could then resort to the more delicate action of the formula given.

It was found that exposed bone, painted with a chlorinated rubber varnish (13), was adequately protected against even strong hydrochloric acid. Consequently, I was able to direct the flow of small amounts of hydrochloric acid with a pipette on a thick clump or coat of matrix, until quite close to the bone, with perfect safety. As successive areas of the bone become exposed, new paintings with the varnish were applied. It is imperative that two coats of the varnish be applied to the bone, and that the minimum of two days of drying be allowed for each coat. It need hardly be mentioned that one should first experiment with valueless fragments of bone and breccia, before using the method on a desirable specimen. For instance, one must learn to judge when the matrix has become so porous that it might allow the strong acid to reach the under-lying bone in particular spots. It is also quite understandable that the composition of breccias from different areas might vary.

(4) We have, then, a method for removing the calcareous matrix from bones in breccia, a method which calls for a minimum of percussion. This advantage is a great one when the bones are brittle, as they usually are. Some time is saved, as other work can be done while the weak acid solution is working on the specimen.

The use of the weak acid solution is particularly advantageous when the bone in question has been approximated either by mechanical means or by the use of strong acid and protecting varnish. The final cleaning need not be done with needles, but can be safely left to the formula. As a matter of fact, when the bone is covered with encasing matrix up to, say, two to three centimetres in thickness, the weak acid solution will render the matrix quite porous, and thus much more amenable to mechanical treatment. The use of the formula is also especially recommended in the case of crushed bones

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[11] Manufactured by Shawinigan Products Co., Shawinigan Falls, P.Q., Canada, and Empire State Bldg., New York, U.S.A. A thinner solution of Alvar is better for impregnating the bone; a thicker one for gluing. In any case, the bone must be absolutely dry before Alvar is applied. Cf. T. P. G. Shaw, 'The Properties of Some of the Polyvinyl Resins as Lacquer Resins', Pamphlet reprinted from the *Official Digest of the Federation of Paint and Varnish Production Clubs*, January, 1937, New York. I am greatly indebted to Mr Frederick P. Orchard, Assistant Curator of Archaeology, Peabody Museum, Harvard University, for advice regarding the use of Alvar and for many other kindnesses.

[12] Angel, 1943. On pp. 4–5, Angel describes a method of complete impregnation with Alvar; a method which involves the use of an aspirator, thereby to ensure the removal of air from the bone and satisfactory substitution of the Alvar.

[13] The varnish used was 'Tornesite', which was kindly provided for my experiments by the Perry-Austen Mfg. Co., Grasmere, Staten Island 5, New York, U.S.A. This varnish proved to be excellent; and I wish here to express my gratefulness to the Company, and my appreciation of their product. The Company supplies a thinner, and so solutions of varying density can be employed, as in the case of Alvar. I used a thinner varnish for the first coat, a thicker one for the second.

whose splinters are commingled with matrix, and in the case of two pieces of a bone which was broken before consolidation of the matrix, the pieces now being united by a thin line of stone, difficult of access by tools. J. FRANKLIN EWING, S.J.

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Livy. *Ab urbe condita libri.*

PIGEON-TOWERS OF THE FAIYUM

After reading James Hornell's excellent article (1) it seemed desirable to call attention to the pigeon-towers of the Faiyum which lies about fifty miles west of Cairo.

As physical anthropologist to the University of California African Expedition encamped from September 1947–February 1948, at Kom Oshim, near the eastern entrance to the Faiyum, I had an opportunity to visit several of the pigeon-towers (*Burg el-Hammam*). The best examples seen were at Jalal Ibrahim Beni Uthman (PLATE XI) near Sennoures and along the shore beyond the Auberge du Lac beside Lake Karun. The former is a five-tiered building with its solitary low entrance in the centre. Inside as soon as our eyes became light-accustomed, we could see the gallant array of circular pigeon nests extending in rows to the highest part of the towers. There were three inner double banks of nesting-boxes and with the six towers, each open to the infinitely blue sky, there was room for about 6,000 pigeons to nest simultaneously.

The owner showed great pride of ownership and muttered that they were really not any financial drawback for they ate off his friends and neighbours. As he said this his sons could not refrain a smile. Our guide, Tolbah from Kom Oshim, assured us that all pigeon-towers in the Faiyum were extremely profitable.

The pigeon-towers of the Iranian Plateau, such as those at Isfahan and Shahreza (cf. my 'Contributions to the Anthropology of Iran', Pls. 132–3, Chicago, 1939), are far more complex structures ornamented with stucco stalactite decorations.

Some buildings at Dzhanbas-Kala in Soviet Turkistan exhibit this same type of architectural construction based originally on pigeon nesting-boxes. Although non-functional, the motif is repeated to give an unusual effect.

HENRY FIELD.

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[1] James Hornell, 'Egyptian and Medieval Pigeon-houses', ANTIQUITY, no. 84, pp. 182–85 and pl. II, December, 1947.

Reviews

PRIMITIVE POTTERY, an Introduction to South African Ceramics, Prehistoric and Protohistoric. *By* J. F. SCHOFIELD. *The South African Archaeological Society Handbook Series, no.* 3; *pp.* 220 *with summary and index,* 14 *plates,* 7 *text figures and end-paper map. Published by the Society at Claremont, Cape, South Africa. Price, to non-members,* 17*s* 6*d* (*cloth*) *and* 15*s* (*paper*).

The South African Archaeological Society is to be complimented on inviting Mr Schofield to write this book and on publishing it when written. Its subject matter is one with which European archaeologists are in general little acquainted, but, should they desire to become better informed, they will find Mr Schofield a trustworthy guide. His book opens with a prologue on the origin of pottery followed by an account of pottery-making among primitive people; and although there may be little factually new in these opening pages they contain so many refreshingly sound observations that they inspire complete confidence in whatever the author may write in his later chapters.

Pottery is a late arrival on the South African archaeological scene and it is classified in this book under five heads: (1) Pottery from Later Stone Age sites; (2) Bushman pottery; (3) Hottentot pottery; (4) Pottery from Iron Age sites; (5) Recent pottery.

The Later Stone Age sites are either open sites or caves, but the pottery from the former may be attributed either to a post-Stone Age occupation or to accidental intrusion. Several cave sites are described, and the curious fact emerges that the pottery found in decorated caves and shelters is broken into comminuted fragments, while that from undecorated caves consists of larger sherds. In both cases the available evidence indicates that the pottery was not made by the cave-dwellers themselves.

There is very little Bushman pottery and its sole distinguishing feature is the use of grass as a filler. It seems that, in his struggle for existence during the 19th century, the Bushman abandoned the art of pottery-making and readily adopted the pottery of other people.

Hottentot pottery is comparatively scarce and little known, chiefly because the nomadic existence led by a Hottentot tribe did not lend itself to the accumulation of masses of debris. Much of it is thin and well-burnt and is characterized by the frequent use of a pointed base and perforated lugs, often internally reinforced. The larger pots were sometimes made elliptical in horizontal section, and in general the pottery forms were such as to be adapted for transport by pack-oxen. Mr Schofield considers that pottery-making among the Hottentots did not antedate the European occupation of South Africa by any lengthy period, and he suggests that the interval between *c.* 1470 and 1770 would allow ample time for the deposits in which it is known to occur.

The section devoted to the discussion of Iron Age pottery is the largest in the book and is prefaced by an ethnological introduction. The term ' Iron Age ' has, of course, a very different connotation in South Africa from that which it has in Europe, and the pottery described in this section ranges in date from the 11th to the 17th centuries A.D. It has been classified according to districts—Rhodesia, Bechuanaland, Transvaal and Natal—and the pottery from Rhodesia includes that from such well-known (and comparatively late) sites as Zimbabwe and Mapungubwe.

To quote from Mr Schofield's admirable summary :

'The earliest Iron Age pottery (that of the proto-Sotho peoples of Southern Rhodesia), called R$_1$, has in its typical forms a carinated profile and was frequently decorated with a comb—features still found in the wares of their descendants. The carinated profile is also characteristic of the Tonga tribes of the Zambesi Valley and the Ronga of Portuguese East Africa. This indicates that the feature has a remarkable antiquity and must have originated before their separation, and perhaps while their tribal ancestors and those of the Sotho were still one people '.

Now it is a fact not without interest that a similar carinated profile is a dominant feature in the pottery from certain sites in the Sudan, amongst them Abu Geili, on the Blue Nile, excavated in 1914 by Mr O. G. S. Crawford on behalf of the late Sir Henry Wellcome.* This pottery may be dated roughly from the 1st to the 8th centuries A.D. or even later, and may possibly be ancestral to that described by Mr Schofield. He mentions elsewhere in his book that the Hottentots encountered by the early European explorers owned a breed of cattle similar to those depicted on Ancient Egyptian monuments, and this may be taken as evidence of influence from the north at some time in the distant past.

The modern pottery of South Africa is discussed under the names of the tribes who make it. Here again there are occasional resemblances between some of the pots illustrated and some modern Sudanese pots, though these similarities may be accidental rather than significant. Mr Schofield himself gives a warning against a too facile interpretation of similarities of this kind when he observes (p. 22) ' the polychrome pottery made at Dhlo-Dhlo and Khami during the 18th century might be mistaken for Hallstatt wares of the 9th century B.C. were its provenance not well established. The present-day pottery made at the Rustenberg Location on the traditional lines of the Peči has all the distinguishing features of La Tène pottery '. Such resemblances, however, are in a different category from those between contemporary pottery made in different parts of Africa.

If only to show that he has read the book and not merely Mr Schofield's own summary of it, the conscientious reviewer must note occasional misprints due to hasty proof-reading, but these are small blemishes on what must for long remain the standard and authoritative work on South African ceramics. F. ADDISON.

THE GILGAMESH EPIC AND OLD TESTAMENT PARALLELS. *By* ALEXANDER HEIDEL. *University of Chicago Press*, 1946. 20*s*.

This is a companion volume to the same author's *Babylonian Genesis*, treating in a similar way the other of the two earliest known and most celebrated Babylonian epics. It falls into two clear halves, the first containing translations (Ch. I) of the Gilgamesh Epic and (Ch. II) of certain other cuneiform texts related in subject to various parts of that Epic : the second half (Chs. III and IV) compares the leading motives of the Epic with the ideas manifested in the Old Testament upon the same subjects. These leading motives are defined by the author as ' Death and the Afterlife ' and ' The Story of the Flood '. They are, of course, upon a quite different footing, for whereas the Epic as a whole is a meditation upon ultimate death, the Flood is only an incident in the drama of mortality. Dr Heidel's translations, both of the Epic itself and of the illustrative texts, are welcome as up-to-date and, in a few cases, the first English renderings of the originals; as for their quality, they rely upon the best authorities, and no doubt a very careful perusal would reveal that they have benefited in detail from the resources of the Chicago lexicographical collection. In one respect they do not give as much as some preceding versions, for no attempt is made to introduce the Sumerian Gilgamesh, a figure of considerably different appearance, projected in a diffuse light, to which the Akkadians gave concentration and focus, though rather spoiling their own effect by admitting an old but inconsequential episode as their Twelfth Tablet. The author might plead for his omission that it is too early to discern the true form of the Sumerian Gilgamesh stories, which are still in process of recovery. But there are isolated passages already known which cannot without loss be neglected in the discussions which occupy the second part of the book.

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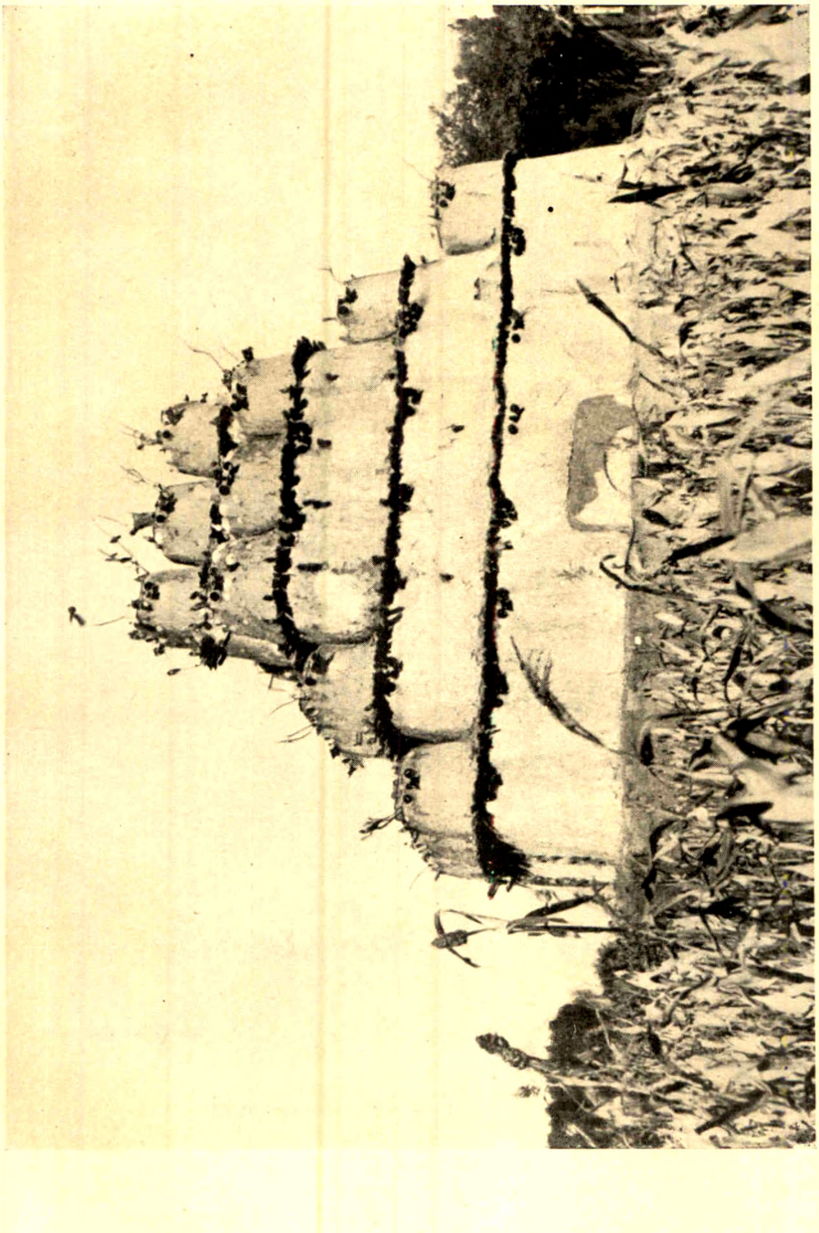
* Now in course of publication. See also *People without a history* by O. G. S. Crawford, *Antiquity*, XXII, no. 85, March 1948.

These discussions are not wholly satisfactory, despite their fulness and care, and this is for other reasons besides their choice of only two subjects, having very unequal importance. Even without the vast extension given to this theme by Jensen (whose work Dr Heidel barely mentions), other parallels have been proposed which might deserve some notice. Surely also the various deposits found by excavators in Iraq and put forward as evidence of a flood, or even *the* flood, are equally deserving of discussion with the burial-customs, and they are evidently of great relevance to the question of geographical origin. Yet even when describing the geophysical theories of Suess the author finds no room for these other concrete if disputable remains. A different criticism might be, that Dr Heidel seems throughout too much bent upon emphasizing at all costs the contrasts between Babylonian and Hebrew ideas and practices. To achieve this contrast he tends to harden both sides into an artificial unity of conceptions, and to stress unduly the monotheistic, enlightened, and moral traits of the Hebrew religion to the neglect alike of the plain traces remaining of beliefs in Israel not always consonant with these, and of the (admittedly less developed) elements of a higher character in the Babylonian. This tendency appropriately reaches its height at the end of the book where, after allowing that ' the Babylonian and Hebrew versions are genetically related ' and that ' the skeleton is the same in both cases ', all the rest of the space is devoted to insisting upon their differences, which, in any view, are so obviously less important than their agreements. Economy has dictated the appearance of this book in reproduction from typescript, but it might have permitted an index. C.J.G.

BRITONS, ROMANS AND SAXONS ROUND SALISBURY AND IN CRANBORNE CHASE. *By* C. F. C. HAWKES: *Archaeological Journal, vol.* CIV, *pp.* 27–81. (Also sold separately by the Institute, Lancaster House, St. James's, S.W.1. *Price* 6*s*).

In the spate of books that is now again pouring out of publishers' presses there is a risk that the still, small voice of true scholarship may be drowned. By scholarship I do not mean unreadable discussions of ancient texts but that much broader, more humane form which it has assumed since the scholar put on a thick pair of boots and learnt the art of extracting history from the soil of England. Symptomatic of this new trend in scholarship was the recent creation of a professorship of European Archaeology at Oxford and the appointment of a field-archaeologist to hold it. Professor Christopher Hawkes graduated in the hard but pleasant school of dirt-archaeology which has grown up in Britain during the last three decades: and his latest publication is a triumphant

PLATE XI



PIGEON-TOWER IN THE FAIYUM

facing p. 108

vindication of the founder of that school, General Pitt-Rivers. That great man brought military precision and method to what, in the 90's, was the playground of *dilettanti.* He excavated prehistoric settlements on his vast property of Cranborne Chase, and published the results (at his own expense) in four classic volumes. So well did he do his work that it is now possible, thanks to his meticulate system of record, to go over the ground again and, in the light of half-a-century's progress in knowledge, to re-assess the results. This Professor Hawkes has done in an admirably lucid monograph. He shows how the General learnt as he went along and, unlike so many Victorians, frankly recorded his previous shortcomings. For instance, the plans of huts were recovered by noting the post-holes still visible in the chalk soil; but the General did not discover this technique until after his excavations had been begun and one ' village ' (Woodcuts) excavated. At the second site (Rotherley) all post-holes were recorded; and by analysing the finds, chiefly pottery, in the light of our *present* knowledge thereof, Professor Hawkes shows that there were several stages in its development. Thus the chaotic and complicated original plan is reduced to an orderly sequence. No greater tribute could be paid to the soundness of the General's method.

The most important result of Professor Hawkes's re-assessment is his demonstration that these native settlements of Rotherley and Woodcuts were not in fact villages but rather farms. It is in fact becoming evident that throughout the British pre-Roman Iron Age farms, not villages, were the typical form of agricultural settlement. We began to suspect this when Professor Bersu revealed the oldest English farm at Little Woodbury near Salisbury, where was a large round hut and some smaller ones enclosed within a single earthen rampart. Then it dawned on us that many of our single-ramparted ' camps ' must also be just single farms round which, to meet some external threat, a protecting bank and ditch had been constructed. Thus they fell into line with the traditional *einzelhofer* of other Celtic lands. Just such a farm, we may be sure, was Lidbury near Enford on Salisbury Plain which, from the air, can still be seen standing in the midst of its plainly visible ancient fields—and there are dozens of others.

Thus it looks as if, in Wessex at any rate, the countryside was covered with fields and farms, the former gradually encroaching on the sheep and cattle ranches whose long lines of boundary-ditch are still to be seen traversing the Plain. These ranch-boundaries have been proved by excavation to be even older, dating from the Late Bronze Age; to which, as Professor Hawkes showed some years ago, the division between Hampshire and Wiltshire probably goes back.

The General's last excavation was that of a Roman building at Iwerne, but he died before he could publish his account of it. Fortunately, however, a model was made by Mr St. George Gray, his chief assistant, who has collaborated with Professor Hawkes in producing an account of it. The outstanding facts are that the stone buildings—there were two, both falling within the Roman period—appear to be the ancestors of the famous Long Houses which still survive in our Far West; and that they in turn superseded an earlier timber structure of pre-Roman date.

Space does not permit more than the briefest mention of the General's excavation of Bokerley Dyke, which Professor Hawkes shows to have been constructed in three stages, all falling within the 3rd century A.D.; and of his disentanglement of the adjacent settlement of Woodyates. It is all of the same first-rate quality as the rest.

O.G.S.C.

THE LOST PROVINCE OR THE WORTH OF BRITAIN. *By* M. P. CHARLESWORTH. Gregynog Lectures, 1948. *Cardiff: University of Wales Press*, 1949. 8*s* 6*d*.

The author of this little book has drunk deep at the Pierian spring whose water intoxicates only if taken in shallow draughts. Descending again to earth (at Cardiff) he unfolds his tale in language learnt in the hard school of practice. Each of the four chapters has a clearly stated objective—why did the Romans ever trouble to conquer Britain and having done so remain here, and what did we and they gain therefrom? In answering these questions all sources of evidence are skilfully used—archaeological, philological and literary; and it is this blend that gives flavour to the result. Technical terms (such as *foederati*) are tactfully explained. The author has historical insight: he rightly points out the implications of *magni* (or in this instance rather *Maximi*) *nominis umbra*—that the bearer ' must have done something considerable and striking, something which made an enduring emotional impression on the minds of Britons, to be enshrined as he is in legend and tradition '. (p. 29). Maxen Wledig must have earned this heroic fame as surely as did Arthur, Ambrosius, Alfred and others. One suspects that Lord Mountbatten may one day be remembered in India for performing not wholly incomparable tasks.

We admit to a certain hesitation in accepting the author's conclusions about some of the alleged benefits of Roman rule. Loan-words from Latin we certainly have, but so have the Irish (as he shows), and it is not apparent what their benefit is, unless the things they denote were also introduced by the Romans, and many were not. The survival of some Roman words is certain, but its frequency and importance have been overestimated. The introduction of trees, fruits, vegetables and flowers was possible without conquest (as the German evidence shows). Box was not one of them, being indigenous. Christianity had to be reintroduced from Rome by Augustine. The art of Rome became extinct in England, being superseded by that of the Anglo-Saxons, which has nothing Roman about it; certain Mediterranean, but not Roman, art-motives came in again with Christianity. (This of course is controversial in certain matters of detail, but surely true in a broad sense). Writing too seems to have become extinct and been reintroduced, but it may have survived in an attenuated form in the Highland Zone. There were watermills in Roman Britain and they must have been introduced by the Romans because there were certainly none here before; but did they survive or were they reintroduced? We would not, however, press these points too far; Mr Charlesworth presents his case, a strong one when all has been said, with force and eloquence.

It seems to us that, in order to prove lasting benefits, a comparison should be made between Britain (or France) on the one side and countries unconquered by Rome on the other, such as Ireland, northern Scotland, Scandinavia and Germany. Such a comparison might suggest that the alleged benefits of Roman conquest were illusory.

We recommend this book most warmly to the readers of ANTIQUITY. It achieves at greater length what it is the aim of every article in ANTIQUITY to do, namely, to present historical and archaeological facts in a popular but authoritative form. O.G.S.C.

ENGLISH POTTERY FIGURES, 1660–1860. *By* REGINALD G. HAGGAR. John Tiranti. *Price* 6*s. net*.

Messrs Tiranti are to be congratulated on a companion volume to their *English Porcelain Figures* by William Ruscoe, for after all the pottery figure is fully eighty years earlier, and to a certain extent set a fashion, which the British porcelain makers were not slow to follow, until eventually the porcelain figures took the lead, and with a few exceptions the pottery figures became a dim reflection of the porcelain aristocrats of Chelsea, Bow and Derby.

But how the pottery figures differ from one another ! They spring upon us in hard white stoneware, perfectly modelled about 1670 from John Dwight's Fulham kilns, and die out about 1840 in those very Victorian figures of Robin Hood, or in those pairs of grave dogs which still adorn the cottage mantelpiece. Between these two dates lie the Stoneware period, 1700–80, the Earthenware period (the so-called 'Astbury-Whieldon' group), 1700–50, the Wood of Burslem period, 1740–1840, and lastly, miscellaneous 'image toys' early 19th century.

Mr Haggar devotes his first four plates to Dwight's Fulham figures, which are in reality individual sculptures in stoneware, and not commercial productions. The following eight plates deal with the very attractive 'Pew Groups' and include several from the late Dr Glaisher's collection, and the well-known lady forming a bell, from the Wallace Elliot collection of about 1740. Slightly earlier is an 'Agate' ware cat, salt glazed, from the same source. Two notable figures in Turkish costume, also in salt glazed stoneware decorated in brilliant green, blue and yellow enamel colours, possibly by William Littler of Longton Hall, bring the series to 1750. It is interesting to note that these same figures were made in hard paste porcelain at Meissen, and in soft paste at Bow.

Turning to the 'Earthenware period,' Mr Haggar deals first with the Astbury-Whieldon figures, nearly all made in Staffordshire. The body was never of Stoneware, but always of earthenware, of a porous character, which required a coating of glaze ; in which both Astbury and Whieldon excelled. Theirs was the heyday of the pottery image maker. The figures are either in coloured clays skilfully blended, or enriched with blotched glazes of purple, brown or green. Plate 15, of a Buffalo, in the Schreiber collection, is an admirable example, and obviously copied from a Chinese model, and its date is about 1750. Two pages further on the author introduces a Deer, likewise in the Chinese taste, about ten years later, by that other outstanding figure modeller, Ralph Wood, the Father of Toby Jugs, one of which may be seen in plate 18. Toby jugs are not strictly pottery figures, but should be dealt with in a volume by themselves. Again the Jasper figures of Josiah Wedgwood hardly seem at home with the products of the peasant potter. For example the Wedgwood 'Ceres' is coldly lifeless beside the Ralph Wood 'Chaucer' (plates 22 and 23) which faces it. Plate 27 is another Toby, a standing version, for which Mr Haggar makes no attribution. In the British Museum there is a similar jug signed 'Walton.' The end of the 18th century saw a spate of cottage figures, mostly classical, a faraway echo of Josiah Wedgwood, which degenerated sadly as time went on. They were decorated in enamel colours, and the best came from the kilns of Neale and Co. (plates 28, 29).

It is curious that Enoch Wood's 'John Wesley' has not found a place in the collection, for it is a finely modelled piece, but the author makes amends with the artist's (for Enoch Wood was a trained artist) 'St. Paul.' The 19th century ushers in John Walton, with his well-known 'bocage,' which has been described as 'the art of the pastry-cook'. He began work at Burslem in 1790 and his factory closed in 1840 (plates 35, 36). With the coming of the Great Exhibition these minor products of the potter faded out, and were replaced by the very conventional figures such as that of 'Orlando' (plate 40). Yet these by-paths of the potter made in the ordinary way of trade, by men far removed from any artistic centre, have a certain charm, which at the same time gives an insight into the peasant life of the 18th and 19th centuries. FRANK STEVENS.

died a few weeks back
Proof corrected by O.G.S.C.

THE BOOK OF SAINTS: a Biographical Dictionary compiled by the Benedictine Monks of St. Augustine's Abbey. *Ramsgate. Fourth Edition. pp.* XVIII+708. 8¼ *by* 5½. *London: Adams and Charles Black,* 1947. 25*s*.

This is a revised and expanded edition of the book, which first appeared in 1921 and was largely the work of Abbot Dom Thomas Bergh, O.S.B., a scholar, whose conservative outlook explains 'his unwillingness to discard ancient legends, especially if sanctioned by liturgical use'. Each entry has been checked and rewritten and the new editors have usually indicated those cases in which the data are uncertain or unreliable.

The arrangement is alphabetical, entries under the same name being given in the order of the respective feast days in the calendar. For each Saint the following particulars, when applicable, are given: name, surname or distinctive appellation, rank (saint or blessed), liturgical group (e.g. martyr, confessor), religious order, present state of cult (see below), date of feast, year or period of death, brief biography. The result is concise, clear and authoritative.

The formal process of canonization is now centralized by a series of enactments and the Roman Martyrology provides an official list of those proposed for the veneration of the church. There are also individuals, such as the Blessed Alcuin of York who have long received a popular cult, but whose 'veneration has never been accompanied by any recognized external sign of cult'. The status of these two classes is not in doubt and the data on which the entries are based are seldom in dispute. But there is, in addition, a very large number of Saints, whose cult is approved, by which the authors 'mean that there is positive historical evidence of a saint having been venerated as such, even if, as in many instances among the Cornish and Welsh saints, that evidence rests solely on ancient church dedications and there is no other official recognition by the Church, such as an approved liturgical office'. This category includes such widely recognized individuals as St. David and St. Teilo. It will be generally recognized that the data on which these entries rest vary very considerably in value. The present work is not based on original research but claims to incorporate the results of recent hagiographical studies. As far as Celtic Saints are concerned, the claim appears to be justified, though we feel that a few further entries might have been purged or pruned with advantage. The spelling of Celtic names is also confusing, forms like Gallgo and Sadwen figuring beside Gluvias and Daniel (for Deiniol of Bangor), generally, but not always, with cross references. A bibliography at the end of each entry facilitates reference to fuller information.

We have noted the following omissions or corrections. The bibliography has no reference to the late Canon Doble's lives of Cornish and Welsh Saints. Aedan—Aidan Bishop of Ferns: the two discrepant entries refer to the same saint. Garmon—Harmon: Nennius provides good evidence of the existence of a Welsh saint, the patron of St. Harmon's, Radnorshire. St. Gwynhoedl, the patron of Llangwnadl, Caernarvonshire, should be included: his memorial was found in the neighbouring parish of Llannor. Malo: Aleth and St. Malo are distinct places, the former probably the old Roman city and diocese, the latter a Celtic hermitage on the Island of St. Aaron (q.v.). Nectan: the Gotha MS. provides a medieval life as trustworthy as many others used. Regulus: the connexion with the translation of St. Andrew's relics is legendary, but this does not discredit the existence of the saint. Sadwen-Sadwrn: his name appears as Saturninus on his memorial in the church of Llansadwrn, Anglesey. Tathai: the principal saint of this name is the patron of Caerwent, Monmouthshire. Teilo: Llandeilo Fawr, Carmarthenshire, the older centre of the saint's cult is not the same as Llandaff, the later seat of the medieval diocese. C.A.R.R.

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Editorial Notes

THE Notes in our last number were written under holiday conditions. The position is now reversed, and these will be read (if at all) by readers on holiday or fresh from it. That—and the usual pressure of space—is why they are so short. We shall revert to the usual length of three pages in December. The pressure is greatest in the review section, and in order to relieve it we are publishing far more reviews than usual in this number. We should welcome comment from readers as a guide to the future. The extra space is obtained at the expense of articles and notes ; is this change approved or not ? We do not contemplate publishing quite so many reviews as this in every number ; but if the review section continues to be popular we might well give more space to it. (We cannot undertake to reply to any letters we may receive but thank the writers now in anticipation).

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The first absolute date from the radiocarbon method has been obtained from wood found in the mesolithic settlement at Star Carr, excavated by Dr Grahame Clark. The date is still confidential, but we may say that it agrees quite nicely with dates already in vogue for the Boreal period. The preliminary report on the 1949 excavations is published in the current number of the *Proceedings of the Prehistoric Society* (New Series, vol. 15 ; Hon. Secretary, Mr T. G. E. Powell, Dept. of Prehistoric Archaeology, The University, Liverpool 3), of which Dr Clark is the Editor. He concludes that Star Carr is 'one of the richest and most informative sites of the Maglemosian culture anywhere in north-western Europe', and that the site belongs to 'an early stage [in its history], one in which upper palaeolithic traditions are still perceptible in the choice of antler rather than bone, and in the methods used for working it ;' and he cites in comparison the technique in use at the famous Late Glacial site near Hamburg. We are thus rapidly discovering exactly how the cultures of the Old Stone Age evolved into those of the New, during the intervening mesolithic period ; and it would be very helpful if specimens of wood from the Hamburg site could be subjected to the radiocarbon process. (The procedure to be adopted was described by Dr Hallam Movius in our last number, pp. 99–101). The excavations at Star Carr are being continued this year and are in progress as we go to press.

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We regret that, in mentioning Dr Grahame Clark's 'Report on Excavations on the Cambridgeshire Car Dyke, 1947' in our last number, under the heading of 'Important New Books and Articles', we carelessly omitted to say where it was published It appeared in the *Antiquaries Journal* for July-October, 1949 (Vol. XXIX, pp. 145–163).

Maps and the Medieval Landscape

by M. W. Beresford

By confining themselves to a small-scale map and broad, easily generalized data, the Ordnance Survey have pioneered in the mapping of historical landscapes. When it comes to the medieval landscape their technique is less happy: for what economic historians (and, I suspect historical geographers) need is not more generalized detail—that they have already in verbal sources. What they look for is more precise detail. The questions which still remain unanswered in medieval economic history are essentially local questions, for which large-scale mapping of local topography would be needed, if the answer were to be presented in map form.

To take one theme as example : the colonization of forest England by the medieval villager. How far did the open-field colonist penetrate ? How much was the physical form of the open-field village modified in marginal geographical areas? Was the individual settler in his forest-clearing an independent man from the very beginning of the strip method of cultivation, which formed the prop as well as the prison of the nucleated village economy ? Did soil types have a strong influence on the speed and direction of the expansion of the village fields outward, and on the slow shaving of the waste ? Did soils affect the propensity to enclose ? Is the present pattern of settlement a high water mark concealing earlier ebbs and flows, or was medieval England more densely colonized than the England of Arthur Young ?

If these questions are to be answered the historian will need good maps. If these do not exist he will have to make them. The research necessary to answer such questions as these will be partly in documents and partly on the ground itself.

To expect help from medieval map-makers will be in vain. The few medieval maps are both small-scale and pictorial. The large-scale map or plan is extremely rare before the mid-sixteenth century (**1**). From that date the craft of surveyor achieved precision and full employment in the service of the many Tudor landlords who were buying or selling land, or in the course of the litigation which itself (from the profusion of documents it evoked) illuminates that litigious age for us.

The large-scale plans of the Elizabethan surveyors exist in sufficient number to whet our appetite for more. In general, they are both accurate and informative and they present us with the open-field village well before its physical dissolution at enclosure. Such maps as those at All Souls', Oxford (**2**), depict a landscape which would have been familiar in its physical aspect to the founder of the College, even if the distribution of names among the strips is not quite in the medieval vein. Other Elizabethan maps catch the village in the act of dissolution—half in open-field and half in newly-hedged enclosures, the hedges in the fields as new as the bright green trees and hedges of the cartographer. One of the All Souls' maps marks the site of a village which had been abandoned during the sheep enclosures of the early 16th century, and it faithfully records the extinguished pattern of the strips upon which the villagers once worked (**3**).

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[1] One of these rare examples (Chertsey Abbey, 1432) is printed in Edward Lynam, *British Maps and Map-makers* (London 1944), p. 16. Curiously enough there is no open field map reproduced in this book. Part of a Tudor village plan will be found at page 21.

[2] These have been transcribed by R. H. Tawney in part in his *Agrarian Problem in the Sixteenth century*. Comparison of these transcripts with the originals reveals many errors of transcription. The best of the All Souls' maps is that of Padbury, Bucks, where almost every acre of the parish is covered with strips.

[3] The half-way map is that of [Weedon] Weston, Northants. The lost village is shown on the Whatborough map : ' the place where the town of Whatborough stood '.

These 16th century maps, few as they are, coupled with the estate plans of the next two centuries give for most English counties a sample of open field landscapes. Lucky counties like Bedford may count their open field maps in scores; less lucky in ones or twos (4). Doubtless more maps still lie in their estate office dust, their value unperceived. But even with the emergence of these into daylight, the open field plans will be no more than a sample. Economic historians are a little nervous of samples, as they see the revisions which have had to be made in the last decade of generalizations based on samples. And samples, from their very nature, will not serve to answer the essentially local questions which have been asked above.

Fortunately the close inspection of open field maps provides a hint of how we may make our own open-field maps for our own local enquiries. The essence of an open-field map is that it should show the position of the single strips of the arable area; the location of land which did not form part of the arable, but was either pasture, meadow, wood or heath; the number and location of the big field units by which the rotation of fallow moved; and the roads and balks which led the peasant to the strip or to the wood or to the town. It is clear that such a map demands something like a 2½ inch scale, if it is to be adequate in its delineation of detail.

In many parts of England such a reconstruction is possible. It hinges on an equation whose truth I have argued elsewhere (5), and which here I must ask to be taken on trust. When I put this equation to people there are usually two responses: some say—as I hope do the readers of ANTIQUITY—' Oh, I always knew that'; and others say, ' Oh, nonsense '. The controversial equation is the simple one that the single strip of the medieval fields is represented exactly by the ridge and furrow of the modern English landscape. From one green furrow, up and over the curved ridge, and down to the next furrow: this twenty-two feet or so is the *selion*, the *land*, the *ridge* of all open-field documents from the 12th century to the 19th. The length of the strip, very roughly approximating to the furrow-length (or fur-long) of 220 yards is the length of the ridge-and-furrow.

There are two simple ways of proving this equation before using it. The first is to take a pre-enclosure strip-map and compare it with the ridge and furrow. It will be found that the strip on the map is represented by the ridge on the ground. Where no such maps survive, a quick inspection of a few fields of ridged land will soon show some point where the ridging passes under a hedge, through a road, or over a canal in a practical demonstration of its priority over these post-enclosure, man-made obstacles to its pattern.

With the equation we may move out into the fields armed with a 2½ inch map. Before long the map will be covered with lines marking the direction of the strips, the boundaries where the strips change direction, and figures showing the number of strips

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[4] The Beds. maps are at the County Record Office. The Essex Record Office has published *The Art of the Map Maker in Essex* 1566–1860 (Chelmsford, 1947). There is no complete printed list of extant open-field maps. In the pre-war *List of Cadastral Maps* the Ordnance Survey indicated those which it had photographed. Negatives and prints were bombed, but prints of many are in the British Museum map-room. In 1888 R. B. Mowat printed *Sixteen Old Maps of Oxfordshire*.

[5] ' Ridge and Furrow and the Open Fields' in *Economic History Review* (new series) 1, 34 (1948). Open field maps are here printed side by side with air photographs of the same area showing ridge and furrow. See also Plates 28–9 of *Wessex from the Air* where Mr O. G. S. Crawford made a similar comparison for Calstone, Wilts.

within each of these blocks—the *furlong* or *cultura* of the medieval field. It will be found easier to outline the bundles of strips and represent their number by a figure, than to draw in each individual strip. If each strip is to be drawn, then a 6 inch or a 25 inch sheet will be needed.

It will be very likely that a number of blanks will be drawn. Some modern fields will bear no sign of ridging at all. If such fields are near a stream or on the far borders of the parish they may well represent land that was never part of the communal arable. If they are nearer the village then their absence may be due to zealous ploughing out since enclosure. In such cases the aid of air photographs may be invoked. From the air the soil discolorations and the crop-marks which result from the old ridging of the ground will be much clearer, and some blanks on the map can be filled.

In those parts of England which have light soils, easily ploughed, the erasure of the pre-enclosure pattern will be most complete. In southern, central and northern England I have found the pattern remarkably persistent and few railway journeys can be undertaken without seeing it.

In cases where no ridging is to be seen either from the ground or from the air it would be rash to argue the absence of open fields without the support of documents. But experiments in marginal areas such as the Pennine slopes, the Yorkshire Wolds, the Wirral, Shropshire and the Peak District have shown that in these areas the erasure of ridge and furrow is slight; and it is precisely in such areas that geographical factors begin to militate against the open field form of agriculture, and where we may see the open field economy adjusting itself to the strict discipline of a narrow Pennine Dale or a steep hilly slope of the Cotswolds.

For naming the *furlongs* the eye must turn from the fields and the air photograph to the documentary sources. These are the minor field-names which occur in almost every medieval charter or land-document. It is in these furlongs that the strip is identified as lying ' between the *selion* of John the Miller to the eastwards and William atte Green to the west, the highway being at the north and the stream called Hallebreck to the south '. One way of collecting such names would be to search every medieval document covering a given area. Such advice is a counsel of more than perfection. But there is a source which helps us to extract the more important names of the two, three or four open fields of the village in which we are interested, as well as a small number of the minor furlong names. These documents, virtually unused as yet by historians, are the glebe terriers: the written surveys in which the territory forming the church lands were recorded and sent to the Bishop. Most dioceses have a bulky collection of such documents: if you are lucky the earliest will date from the 1570's; and in most cases the series will be certain to begin about the middle of the next century (6)

In such documents the land described will, in most open-field villages, be exactly like other land there. It will consist of many scattered strips, and in order to describe them accurately the terrier will have to name the Field in which the strip lies, the furlong where it is to be found, and its position in the furlong—either whose strip is lying next to it on either side, or its position in relation to the bounds of the furlong—' being ye seventh and ye fifteenth lands from ye northewards '. In very many terriers which I have examined in the Lichfield, Coventry, Worcester, Lincoln and York dioceses the eye is aided by a tidy habit on the part of the 17th century clergy: the parchment is divided into columns, one for each Field, and the field name is written boldly at the head

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[6] I have described and transcribed the Leicestershire terriers in W. G. Hoskins (ed.) *Studies in Leics. Agrarian History* (Leicester, 1949), pp. 77–126. The Yorkshire terriers are dealt with in *Yorks. Archaeological Journal*, 1950.

of the column. Within each column a line divides furlong from furlong, with the furlong name indented in the left margin. With such a document the essential names can be extracted in a matter of seconds.

We now have some names on our map, and in fact rather more field names and detail than a modern 6-inch map will give us for our modern landscape. The plotting of the strips will probably have revealed the fossilized road-ways of the open field village, the *balks* running between the furlongs. Some of these will have been converted into modern lanes, whose sudden right-angled turn reflects the arrival of the furlong-end. Others will be (significantly) footpaths on the Ordnance Map. Others will be broad grass elevations threading their way among the fields, in step with the ridges and furrows which they once served, but out of step with the modern hedges and ditches which have been laid across them.

Our reconstruction has so far ignored the village itself. In the nucleated village the houses were grouped at the core of the fields. The farm-house out in the fields is a modern creation, its enclosure-born date often signified in its historical name—Waterloo Farm, Quebec Farm, Wellington Farm.

When we come to the village we are badly hampered by later development. The 17th century saw the arrival in the village of a firmly-built brick, wooden or stone cottage which has overlaid the medieval cot. Many villages have expanded along the street but it is impossible to say from looking at houses exactly how far the medieval village ran. We shall have the site of the medieval church, and if the restorers have not been at work we shall have something of its structure. We may have the medieval manor house, and if we have not the mill itself we shall probably have its site in a field-name or in a mound and ditch.

To obtain a precise picture of a medieval village laid out along its streets we should be able to call in the archaeologist. But the traditional interests of English archaeology have not laid in the direction of medieval villages, and it must be said in their defence that modern villagers have never taken too kindly to the suggestion that their house-floors should be removed and the garden pitted in the pursuit of knowledge.

The gap is within sight of being filled by recourse to sites of villages which have been abandoned. It is never quite as useful to dissect a corpse as it is to dissect a living body, but the very fact that we have a corpse on our hands provides us with a fresh set of historical questions to be answered : why did the village die ? Indeed the profusion of lost village sites which can be traced once a thorough search is begun, makes it necessary to pose another question : why did so many sites find themselves abandoned ? Or in another form, that may be put : how many villages *were* there altogether in (say) 1250 ?

Such abandoned sites have preserved for us the village streets : these are now deep depressions through the grass, their right angles a little smoothed by the grass which grows over them and the soil which has been blown and washed into them. But it is not necessary to go into the air to see them. Once recognized, they are very clear to the ground observer. The buildings of abandoned villages have rarely remained above ground. We found the floor of the church at Stretton Baskerville (Warws.) a foot below the turf, with the broken floor tiles and the roof slates which had fallen on them (7). For the church walls we had to go to a nearby barn, where some of the worked stone had been used ; some had been used to dam the outlet of the fine set of medieval fish-ponds ; and some were in use at the manor House some half mile or so away as the sides of a sunken tennis court. In other villages, such as Cestersover (Warws.) the church has been used

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[7] I am describing this and other Warwickshire sites in the Birmingham and Midland Archaeological Society's *Transactions*, vol. 66 (in the press).

as a barn ; in others, like Wharram Percy (East Riding) it survives as the only building in the grassy fields where its parishioners once lived (8).

With building stone valuable, it is not surprising that any worked stone from lesser buildings has been carried away to other villages. What trial excavations have revealed is very little stonework other than rubble foundations or stone hearth-bases. No large buildings, apart from churches, have been excavated, although at Wharram Percy a particularly clear air photograph is an excavator's blue-print of a many-roomed house. What remains of cottages above ground is little more than a hollow depression or the raised platform on which a house stood. The boundary mounds of cottage, toft and croft, field and garden are rather more clear, and in many of them the right-angled turns are virtually unobscured by erosion.

It is to such lonely fields that we must go if we are to see the unimpaired ground-plan of the medieval village. In most counties the journey need not be all that far. The abandoned villages of Lincolnshire were listed by Canon Foster as long ago as 1924, and one of them was pictured in an air photograph by O. G. S. Crawford. A site near Oxford was excavated before the War, and in 1945 Dr W. G. Hoskins published his list and analysis of the Leicestershire villages. His map locates 49 sites and others have been added since (9). In Warwickshire I have completed a similar survey, and about sixty have been mapped. Similar surveys of the three Yorkshire Ridings are well on the way, with indications that the joint total may exceed a hundred villages and hamlets. A rough glance at Buckinghamshire, Northamptonshire and Oxfordshire indicate that there is abundant material there also ; and sites in Derbyshire, Durham, Nottinghamshire and Cambridgeshire have been noted.

In the production of such maps as I have described, the selfish need of economic history research has been the main goad. (10) The method has been described here in the hope that it may be applied by others in other parts of the country for this and for other purposes. For enquiries where large-scale detail of the medieval landscape is needed I believe that this approach, combining field-work, air photographs and documentary evidence will provide something of a consolation where open field maps are missing. A 2½ inch map with such detail added will be able to answer some of the questions which we would put to a Tudor survey : but it is unlikely—at least in my hands—to have the pictorial qualities which make such maps as those at All Souls' or at Chelmsford such a delight to handle.

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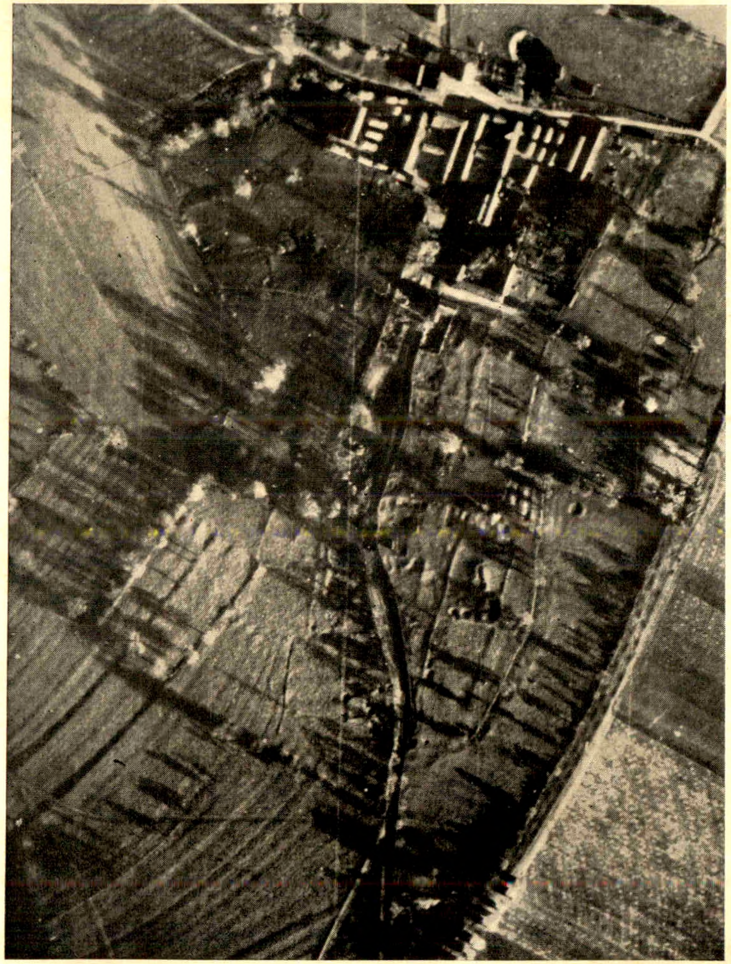
[8] The air photograph references of these two sites are : Wharram Percy, CPE UK 2593/3030 ; Cestersover, CPE UK 1925/2005 and 4003. I have given short accounts, with photographs of both the Ridge and Furrow and the Lost village themes in two articles in '*Country Life*'—'What is Ridge and Furrow' (4 March 1949) (Plates of 1593 map of Weston, Northants; air photographs of that area and of East Newton, E.R. Yorks.; ground photographs of Ilmington, Warws.; map of 1737 of Allestree, Derbys.). 'Tracing Lost Villages' *ibid*, 15 October 1948). (Plates of 1586 map of Whatborough, Leics., with air photograph of the site; air photographs of Cottam, E.R. Yorks.; Upper Radbourn, Warws.; Cestersover, Warws.). Two other air photographs of field-patterns will be found in the article cited above in the *Yorks. Arch. Journal*, 1950 (of Wighill, W.R., and of Hornby, N.R.)

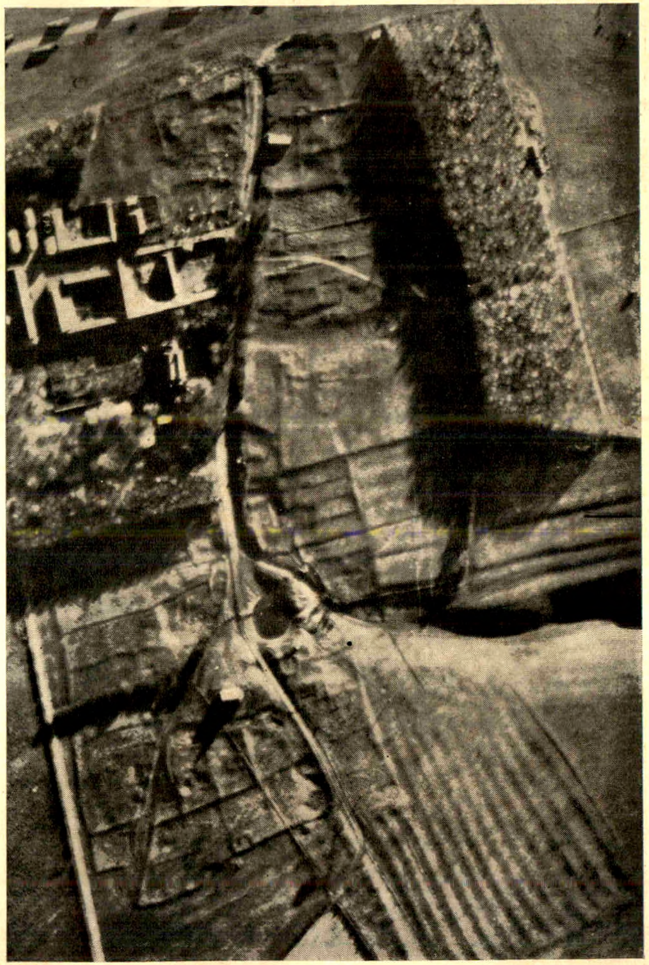
[9] Lincoln Record Society, volume 19 (1924) *The Lincolnshire Domesday*. O. G. S. Crawford in *Antiquaries Journal*, 1925, p. 432, for photograph of Gaisthorpe. The Berks. site is described in *Oxoniensis* v, p. 31 (1940) by Bruce Mitford. Dr Hoskins' article is in *Trans. Leics. Archaeological Society*, XXII, 242.

[10] Smaller scale maps could be used for the recording of manorial boundaries, distribution of monastic property, manor houses, castles, mills, etc. I believe that Colonel Drew is compiling such a tenurial topography.



THE DESERTED SITE OF TOWTHORPE (E.R. YORKS) WITH THE STREET, HOUSES AND CLOSES, AND THE MODERN FARM OVER-LYING PART OF THE SITE. THE MEDIEVAL PLOUGHLANDS SWEEP UP TO THE SITE

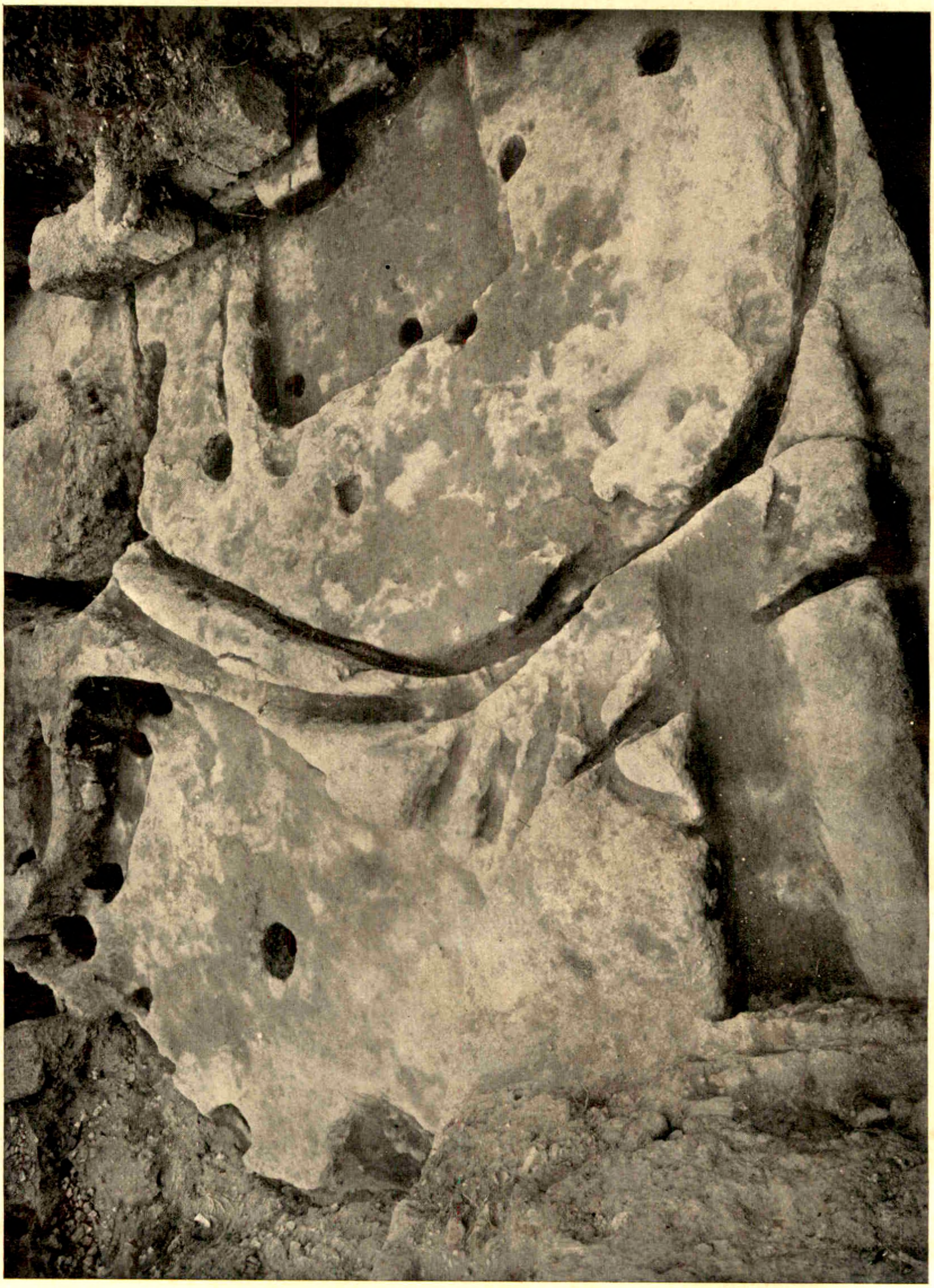
(O.S. Grid. ref. 44/900629)



THE DESERTED SITE OF COTTAM (E.R. YORKS) SHOWING THE CROFTS BEHIND THE HOUSES, THE STREETS AND THE FIRST *FURLONG* OF THE OPEN FIELDS

(O.S. Grid ref. 44/992648)

PLATE II



HUTS ON THE PALATINE HILL, ROME, FOUND 1948

Huts on the Palatine Hill, Rome

by S. M. Puglisi

THE discovery of primitive hut-foundations within the walls of Rome is an event of some importance, throwing light on the prehistoric origins of that great city. These hut-foundations were found during excavations carried out under the general direction of Professor P. Romanelli in the area of Germalus, and my own share in the work was concerned with the pre- and protohistoric remains found (1).

Earlier observations by Vaglieri on the Germalus (2) and by Boni under the Domus Flavia (3) had, in fact, proved the existence of a village of the Early Iron Age (1st and 2nd phases of Latian culture) extending over both the Palatine hill-tops. The main objects of the present excavation were to find an area undisturbed by the intensive building of republican and imperial times, to make a stratigraphical survey, and to re-examine the surface remains interpreted by Vaglieri as those of a cemetery (4). The heated controversy between Vaglieri and Pigorini about the nature of the finds did not settle the problem, but merely brought the excavations to a standstill. Vaglieri's contention was refuted both by a re-examination of the archaeological material in the Antiquario Palatino and by the remains found by Boni under the Domus Flavia. Both suggested domestic activities—the remains of baked clay ovens and potsherds, wattle and daub fragments of hut walls (Boni excavations), and food refuse such as broken and burnt animal bones.

The surface on which the huts under the Domus Flavia were built was the upper layer of the fluvio-lacustrine deposits which rest on the tufa rock (5). Although Boni's photographs are difficult to interpret, they do show some post-holes. But the uncompact nature of the deposits evidently made observation more difficult than on the Germalus site where, on the lower crest of the hill, the upper deposits had been removed by the action of rain, exposing the hard tufa surface on which the primitive inhabitants could build their huts (6). Channels dug round the huts (PLATE II) were evidently designed as a protection against rain-water. A similar device was observed by Mengarelli at Satricum (7) and by Rellini at Punta Manaccore (8). Remains of two such dwellings and parts of a third were found by Vaglieri. On the Germalus site the deposits fortunately were still undisturbed, owing to the building (during the middle republican period) of a wall of large yellow tufa blocks. Within this large hut, whose structure was later found to be almost completely preserved, excavation was carried out with strict attention to stratigraphical

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[1] See my Report read at a meeting of the Istituto di Antropologia, 29 May 1948.

[2] *Not. Scav.*, 1907, 185, 264, 444, 529.

[3] Report partially published by M. Marella Vianello in *Antichità*, I, 1947.

[4] See *RAL.*, XVI, 2; XVII, 1; XVIII, 5.

[5] Section published by De Angelis d'Ossat in Per la ricerca del Lupercale, *Bull. Comm. Arch. Com. di Roma*, LXII, 1934, 79.

[6] The geological aspect of the recent excavations has been dealt with by G. De Angelis d'Ossat.

[7] G. Pinza, *Mon. Ant. Lincei*, XV, 1905, 480 (Mon. primitivi di Roma e del Lazio antico).

[8] *BPI*, LIV, 1934, 12–13.

method. The results provide the first objective evidence for the succession of cultures on the hill from its first occupation down to the 3rd century B.C. Of the four strata observed in this area the first two belong to the 1st and 2nd phases of the Latian Iron Age, which are already well known from burials in the Alban and Roman hills and particularly from Boni's famous excavation in the Forum cemetery (**9**). The following description deals only with the stratigraphy of the earliest archaic period.

Stratum 1 is a thin layer of clayey sand with organic remains, immediately overlying the hut floor. It yielded wattle and daub fragments, charcoal and ash marking the site of a central hearth, small fragments (probably broken up by walking over them) of burnished and unburnished hand-made pots with incised geometric ornament. There were also remains of good debris. The probable date is about 800–700 B.C.

Stratum 2 is thicker and was deposited after the hut was disused. The soil has a greater organic content (refuse dump ?). Most of the pottery is still not wheel-turned, but the upper ring of the double handle is more developed, and twisted handles and red-burnished ware appear. Decoration consists of finely incised straight or curvilinear designs including semicircles and double spirals. Some *bucchero* ware occurs, together with local imitations of early Greek forms (oinochoai, skyphoi, kilikes). Significant are fragments of proto-geometric painted pottery undoubtedly imported from Hellenic or hellenized centres (proto-Corinthic style), and fragments of the Italo-geometric ware equivalent thereto. This stratum is quite homogeneous and full of well defined fragments, and it corresponds to the 2nd phase of the Latian cemeteries, typified by Riserva del Truglio (**10**). The probable date is about 700–550 B.C.

Strata 3 and 4 contain objects of the republican age; below them is a transitional level with some remains of the 2nd phase.

The attribution of this occupation of Germalus and Palatium to hut-dwellers of the earliest Iron Age is based upon the following facts:—(1) the agreement of the material from the first two strata with that from the Forum cemetery which is clearly to be associated with this village: (2) the presence in the large hut of these first two strata: (3) The structure of the dwelling found during the recent excavations. This recently revealed hut is of sub-rectangular shape and measures about 3.65 by 4.80 metres; it is dug out of the tufa which here is marked by *pozzuolana*. The traditional half-sunk Italian hut typical from neolithic and eneolithic times (**11**) is here still in use; the depth of its sunken area ranges from 0.50 to 0.90 metres. The entrance faces south and has a kind of step. Round the edge of the hut are post-holes dug to hold up the roof-supports.

A large hole found in the centre of the hut solves once and for all the problem of the structure of Latian dwellings, imaginatively reconstructed on the basis of the pottery hut-urns (**12**). Confidence in such reconstructions was weakened by the conventional character of the pottery models which also naturally, being meant to hold ashes, gave no clue to the internal arrangements of the originals. Now, as a result of collaboration with Mr A. Davico, an architect, the general similarity in plan and structure between the Germalus hut and those represented by the Early Iron Age Latian hut-urns has been fully established. Outside the Germalus hut on either side of the entrance are two holes for posts supporting a projection of the sloping roof, or perhaps for a small porch such

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[9] *Not. Scav.*, 1902 (3); 1903 (4, 8); 1905 (6); 1906 (1, 7); 1911(3).

[10] U. Antonielli, *BPI.*, XLIV, 1924.

[11] Cf. G. A. Colini in *BPI*, XXXII, 1907 (Scoperte arch. nella valle della Vibrata); U. Rellini in *Not. Scav.*, 1925 (Scavi preistorici a Serra d'Alto).

[12] *Not. Scav.*, 1893, 204 ff.

as that shown in an urn from Campo Fattore now in the Prehistoric Museum, Rome. A cutting on the right hand side (looking outwards from the hut towards the entrance) corresponds exactly to a window seen in the same position in many Latian and South Etrurian hut-urns. The stability of the turtle-like roof as is shown by the hut-urns was secured by means of a central pillar of considerable size, as may be seen from the diameter (0.45 m.) and depth (0.55 m.) of the hole. The hut-urns show a horizontal beam in the roof much shorter than the diameter of the hut; this was evidently designed to support the weight of the roof. The remains of the hearth were fairly close to the central pillar, but the risk of fire was avoided by the use of a closed flame oven such as was in use already in the Bronze Age by the Apennine people (**13**).

We may conclude that people with a uniform culture were settled in various places in Latium, including the Roman hill, during the first part of the Iron Age; and it is not necessary to accept the traditional priority of settlements in the Alban Hills (**14**). On this point the evidence from cremation and inhumation burials in the Forum agrees with that from our village. Physical anthropology can tell us nothing about the cremating peoples, but it recognizes the Mediterranean characteristics of the Latian inhabitants who practised inhumation (**15**). Moreover the practice of cremation—widespread in Europe and appearing first in the Po valley during the Bronze Age—probably spread rather by cultural diffusion than by ethnical migration.

The spread of the hut-urns throughout west central Italy, and their points of difference from those made elsewhere (**16**), and above all their presence in regions of mixed burial ritual, suggest that the adoption of cremation was general and not necessarily connected with a particular culture, much less with a particular ethnic group. We may recall the use of huts as burial-places by the Mediterranean peoples of Italy (**17**); the use of hut-urns might well be a sort of graft on this practice, just as megalithic burial-chambers might be regarded as 'houses of the dead (**18**)'.

ABBREVIATIONS

RAL = Rendiconti dell' Accademia dei Lincei (classe scienze morali).
Not. Scav. = Notizie degli Scavi.
BPI = Bullettino di Paletnologia Italiana.

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[13] Cf. U. Rellini, *Not. Scav.*, 1931, 184 (Nuove ricerche al pianello di Genga e nella Gola del Sentino).

[14] P. Ducati, Come nacque Roma (Rome, 1939), p. 144.

[15] On this see G. Sergi, Italia: le origini, ed. Bocca, 1919; S. Sergi, Antropologia laziale, published in 'Le scienze fisiche e biologiche in Roma e nel Lazio', Ist. Studi Romani, 1933; G. Sergi, Da Alba Longa a Roma, Turin, 1934.

[16] See Behn, Hausurnen, Berlin, 1924.

[17] U. Rellini, Scavi preistorici a Serra d'Alto, *Not. Scav.*, 1925, Plate XVIII, 1, 2.

[18] Gordon Childe in ANTIQUITY, XXIII, 1949, 135.

What *matters* in Archaeology?*

by R. E. M. Wheeler

I SUPPOSE that everyone now and then sits back, whatever his occupation, and asks himself or herself the question : ' Is what I am doing worth while ? Whither am I going ? Is my journey really necessary ' ? As often as not, we muff the answer and indulge in a comfortable sophistry. Save in those restful operations, such as war-service, where we have surrendered our personality and merely march in step, we are rarely honest with ourselves. We live largely, I am afraid, on self-flattery and evasion. Our life is but a sleep and a forgetting. As scientists, for example, we may say that *all* scientific knowledge, *all* scientific discovery, is worth while, and whatever we may be doing as scientists is *ipso facto* justified. Science is an end in itself.

If we argue in that fashion, we are of course evading the point. Everything we do in life is selective. We select our boots and neckties; within narrow limits we select our lunch and our residence. As I look across this room, my eyes are instinctively selective: at the present instant, they have selected the third person from the right in the fourth row from the back. As scientists, our life is founded on selection and decision. We like to think that that selection and decision are objective and impersonal. What fools we are!

For let us never forget that the subject of our particular science is Ourselves. We falsify our approach at the outset by calling ourselves Homo Sapiens. We then proceed to demonstrate what a fine fellow Homo Sapiens is; how age after age he has been stimulated by opportunity or adversity from triumph to triumph, from revolution to successful revolution; from hand-axe to atomic pile. Even if the task means sitting down as authors to 12 volumes of typescript, we do not shrink from it. True, the Chinese set a term to this sort of thing : 'All the wisdom of the world', says the Chinese proverb, 'is contained in a cartload of books'. But so much moderation is the exception to the rule.

As archaeological scientists, our subject is Man, a subject which, being Men ourselves, we can never fully objectify. Our science is of all sciences the most subjective and selective. Within the far-off boundaries of ultimate causes, the geologist can comprehend and objectify his stone; a simple and obvious natural law prevents the human mind from an equivalent process of self-comprehension. We obviously cannot be inside ourselves and outside ourselves simultaneously. However scientific certain of the mechanism of archaeology may be, archaeology must remain itself essentially an inexact science, subjectively selective.

I do not propose, however, to develop this semi-philosophic aspect of our discipline this afternoon. My purpose is to take more specific problems, with, I hope, a more immediately practical trend. I have asked the question—and please note the question-mark— 'What *matters* in archaeology?' not as a philosopher but as a working archaeologist. I do not expect to answer that question, but can console myself with the thought that questions are often more valuable than their answers.

Our subject is Man, and I repeat that truism because it is the only easy thing we can say about archaeology. Beyond that point, paths diverge with a bewildering complexity. What do we mean by ' Man ' ? The amiable Dean Swift described him as ' a forked straddling animal with bandy legs '. The greatest philosopher of clothes, following the Dean's tradition, regarded Man as ' a forked Radish with a head fantastically carved ' ; though in a rare moment of charity that same philosopher conceded that he was ' also a

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*An address to the Council for British Archaeology at Burlington House, London, 10 July 1950.

Spirit, and unutterable Mystery of Mysteries'. That definition may have thrown dust in the eyes of Chelsea in the 1830's, but it no longer satisfied German realism a generation later. 'Man is what he eats', said Feuerbach; and I am inclined to think that there is a great deal more in that definition than might appear at first sight. The man for example, who fills his belly with rice is a different species, almost a different genus, from the man who fills his belly with wheat. His whole reaction to the problems of life is a world apart. But let us nevertheless brush aside Feuerbach's definition as a heavy Teutonic witticism and turn to another statement of German materialism. It takes the form of a protest against those idealists who believe that man comes trailing clouds of glory from God which is his home. It reads: 'Do these gentlemen think that they can understand the first word of history as long as they exclude the relations of man to nature, natural science and industry? Do they believe that they can comprehend any epoch without grasping the industry of the period, the immediate methods of production in actual life? . . . Just as they separate the soul from the body, and themselves from the world, so they separate history from natural science and industry, so they find the birthplace of history not in the gross material production on earth, but in the misty cloud-formation of heaven'. Or, to quote again from the same thinker: 'In changing the modes of production, mankind changes all its social relations. The hand-mill creates a society with the feudal lord; the steam-mill a society with the industrial capitalist. The same men who establish social relations in conformity with their material production also create principles, ideas and categories in conformity with their social relations . . . All such ideas and categories are therefore historical and transitory products'. As my listeners may have guessed, I have been quoting from the writings of a certain Karl Marx who, I understand, was a continental archaeologist of the Gladstonian era. In all this there is of course a lot of the old problem of the relative priority of the hen and the egg, to which are added all sorts of complications arising out of chicken-feed. The whole thing is a tangle, and it is to me sometimes a refreshing thought that at the end of his life Karl Marx was himself moved to proclaim 'At any rate I am not a Marxist'.

Well there we have it. Man is (a) a bandy-legged animal, (b) a bagful of star-dust, (c) a Frankenstein victimized mentally and socially by his own handiwork. There are probably 101 other definitions but these will suffice. I have no doubt that they are all true. But you see how difficult this diversity makes things for us! As a Council for British Archaeology we are interested in policy. Indeed, with the active help of Professor and Mrs Hawkes, Professor Piggott and others we have produced a most brilliant statement of present achievement and future needs, and I hope that we shall produce many more. The statement of present achievement will retain a permanent place in our archaeological literature. Its corollary, the statement of future needs, is a delightful and entertaining *jeu d'esprit* but doubtless as evanescent as its predecessors in the past decades and its successors in time to come. However we may try to steer the future, the archaeological biologist will calmly continue his evaluation of Man in the jellyfish-to-vertebrate succession; the idealist will continue to discover spiritual qualities in Romano-British sculpture; the materialist will scrape the pollen off his fish-hooks and wonder why the barb was placed outside rather than inside. Upon the gates of the future we men of the past may hammer in vain.

All this may sound a little disheartening, but I at least am not disheartened. Having in my last paragraph cut the ground away from beneath my feet, I propose to hitch my wagon to a star and carry on. In all this welter of aim and method, is it useful or even possible to say what matters more and what matters less? I remember hearing a distinguished pre- and proto-historian, who may or may not be present here this afternoon, remark that medieval archaeology in this country was ' a worked-out mine '. I remember also how on one occasion in this room in my youth I got up to read a carefully prepared paper on a new prehistoric beaker which could not fail, I felt, to make a deep impression on the Fellowship before me. As I began, a husky voice came from the midst of my audience, uttering, in terms of withering scorn which I cannot simulate, the words ' *More Beaker-folk* ! ', followed by much scuffling as the owner of the voice struggled for the door. Well, there at once we have two points of view as to what does or does not matter. To this aspect of the problem I shall return. Again, there is the interesting question as to *standards of research*, particularly of excavation. As a young student, I was briefed by a former President of the Society of Antiquaries before undertaking a certain piece of research, and I remember his saying to me 'Of course you mustn't pay any attention to Silchester—it was dug up like potatoes '. And a few weeks ago, 5000 miles from here, I was sitting on the red-hot brickwork of a famous prehistoric city in the Indus Valley with a young archaeologist fresh from Oxford, after checking up with him certain published plans of previous excavations on the site. Everything that we had checked had been found wrong—wrong in detail, often wrong even in general sense. He was properly scornful, and his scorn was of a kind to which I have myself in fact given expression on other occasions. At almost every point our predecessors had lacked, and notably lacked, that degree of scholarship which, since Pitt Rivers began his work in 1880, has constituted the minimum standard of archaeology in the field, and is even more vital than scholarship in the study because normally it cannot afterwards be corrected—the evidence being largely destroyed, of course, in the process of discovery. Nevertheless, I was moved to take up for a moment the defence of these unscholarly folk, and my argument is relevant to our problem this afternoon. It is all a question of *What really matters*.

Let us for a moment consider the situation as it originally confronted the excavators of Silchester on the one hand and Mohenjo-daro on the other. First, Silchester. In 1890 none of our Romano-British towns had been excavated ; we knew almost nothing of their plans and buildings, of their material and sociological make-up. We could not, in fact, begin to discuss the economy and sociology of Roman Britain. What was wanted, and within a measurable space of time, was just such a picture as the excavators of Silchester proceeded to give us. The picture was of course both synthetic and incomplete ; but thereafter we knew certain fundamentally important things about our Roman towns and could begin to fit them into our general scheme. It would have taken ten Pitt Riverses ten times as long as Pitt Rivers's two decades to have dug the site accurately in depth, and meanwhile much else that was now due would have been kept waiting : the whole accelerating progress of Romano-British studies would have been held up, the impulse lost. It is a sound military axiom that a second-rate plan carried out in time is preferable to a first-rate plan executed tardily ; and, applied with caution, this axiom may be apposite on occasion to scientific research. It was certainly apposite to Silchester, however little the matter may have been understood by the excavators themselves.

Similarly in respect of Mohenjo-daro. Before 1921 the very existence of the most extensive riverine civilization of the ancient world was unknown. Then the discovery of a few seals and potsherds and brick walls hinted at a new world. Now what would have happened had you or I been concerned with the matter ? I think we can guess. We should have dug a number of careful sections. From them we should have abstracted a reasoned grammar of Indus pottery, beads, seals and implements. We should have uncovered here and there a few buildings which might or might not have been intrinsically important. We should, in short, have produced an adequately documented Indus Valley Culture.

But what in fact did the excavators do? Practically none of these things. After 12 years or more of work on an extravagant scale, we still have no notion of the development in depth of the Indus Valley Culture. We know almost nothing of the development of Indus Valley pottery or seals or other equipment. We are told that age after age this remarkable culture preserved an uncanny uniformity. The reason is of course that the excavation was almost completely unanalytical. Stratification simply was not understood. All the colours in the paint-box were mixed up, producing a uniform muddy brown.

But what the excavators, with all their up-Guards-and-at-'em methods *did* do, was something that in a sense transcended all this. They built barracks on this desert spot, imported whole regiments of hillmen with their families, and turned two thousand of them or more on to the dusty mounds. Day after day, building after building emerged from the soil, streets and lanes fell into place in a great town-plan, wells and drainage on a scale altogether new to knowledge began to build a picture of an ordered polity that was in the fullest sense an evolved civilization. The excavators found a culture and left a civilization. They did many stupid things, many criminally unscientific things, their records are often childish to a degree, and even within their own limitations they did not fully appreciate what they were finding. I need not go into the details of their sins of commission and omission. As Tacitus said of Julius Caesar's venture in Britain, they may be said to have *shown* the Indus Civilization to us, certainly not to have *delivered* it to us. But they at least did that, as you or I would probably *not* have done. It was all very scandalous, but we have to admit that they, like Schliemann, added a chapter to the history of civilization where we, the grandchildren of Pitt Rivers, might have added a paragraph to a catalogue of cultures. In that particular phase of knowledge, they gave us what primarily *mattered.*

I am well aware that, in applauding Silchester and Mohenjo-daro, I am trespassing dangerously beyond the borders of scientific morality. I am commending crime because it happened to be successful. Let me make it clear that in neither case, either at Silchester or at Mohenjo-daro, am I suggesting that any special merit accrues to the excavators for the methods and policy which they adopted. In fact they knew no better. They had, as I have said, no understanding of digging in depth, of stratification, of analytical excavation. I remember one of them visiting me at Maiden Castle, when, in that closely interleaved site, I was digging a small area with a teaspoon. 'What you want', said my visitor robustly, 'is 300 men to whip the whole of the surface off'. Years afterwards, standing on the eviscerated mounds of his Mohenjo-daro, I recalled his words with a new understanding of his mind.

Nevertheless, hidden away in all this immorality is a moral which is worthy of our attention. I have mentioned Maiden Castle, and am reminded that, in publishing our work there, I expressed regret that circumstances had prevented us from recovering any considerable portion of the ancient town-plan of the place. I think I suggested Hod Hill as likely to be more suitable for a project of this kind. More recently, the authors of our own *Survey and Policy of Field Research* have, I am glad to see, returned to the charge with the remark that at least one Early Iron Age site in each main region of the country, *excavated totally*, 'will now be of more value than merely to sample a number of sites in the same time instead'. True, on the following page they also recommend selective excavations, but the main point stands. We do in fact need more *complete* pictures of our ancient habitations, villages and towns: more long-term work. And in other countries the need may be described as desperate. At the best, we can only hope to find a disjointed fragment of man's achievement. Let us at least ensure that that fragment is as nearly an entity as circumstances permit. We cannot, I am afraid, pretend that a mesolithic hearth in Surrey or an Iron Age hut in Dorset has the same magic property that Tennyson ascribed to his notorious 'flower in a crannied wall':

> 'Little flower—but if I could understand
> What you are, root and all, and all in all,
> I should know what God and man is'.

The mere archaeologist requires something more substantial on which to base the cosmos. 'Those who believe', says the wise Polybius, 'that they can obtain a proper general understanding of history from a study of its fragments are much in the position of those who might think that they could comprehend the life and beauty of a living thing from the *disjecta* of its once living body'. If we must have fragments, let us at least enlarge them to the best of our ability. There is at present a tendency in our archaeology to *fidget* too much—doubtless a legacy of War but none the more desirable for that. What we want now is more solid, steady, big-scale work; carefully trained for, planned for, and carried through to a logical conclusion.

How is this to be effected? In this country, even after 5 years of peace, there is no insuperable difficulty, provided that the determination is present. There is certainly no insuperable *technical* difficulty. In Asia, where so much remains to be done on some of the main arteries of human achievement, the problem is less straightforward. On a relatively small scale, I have just had experience of it, and there is one technical difficulty which calls for nice adjustment. Setting aside the summary methods to which I have referred, we are presumably left with the deliberate scholarly methods of the Pitt Rivers tradition. But, for the reasons already given, these methods, undiluted, will not as a rule meet the Eastern problem. There the accumulation of soil and other material is on the average five times as great as in this country, and if a reasonable amount of horizontal excavation is to be accomplished—if we are to uncover enough of the plan to *matter*—the vertical digging must be speeded up. For example, at the end of my first month's work in Pakistan this year, I sent back 12 bullock-wagon loads of *selected* pottery to base, but still had no idea what I was digging. This was not a bit like Cranborne Chase, and we (Mr Leslie Alcock and I) had to evolve under duress a system of digging which would not, I hope, be practised at Dorchester by the Oxford University Archaeological Society. As we proceeded from layer to layer we had to make a quick decision on the spot whether seriously to stratify or not; and I have come to the conclusion that, in circumstances such as those in which we found ourselves, this procedure is necessary and right. Had we adopted a more leisurely method and stratified everything, we should have accumulated an entirely unmanageable mass of nominally documented material from strata of which we could not adequately discern the significance, and we should have been little the wiser. We still had, as I say, no notion what we were digging up. It was only within the last 9 days of our work that the meaning and relationship of the immense structure on which we had landed suddenly revealed itself. Had it not been uncovered almost in its entirety, it would have remained a mere shapeless and meaningless accumulation of brickwork, unrelated to the citadel on which it stood and adding nothing, literally nothing, to our knowledge of the civilization to which it belonged. As it was, by limiting ourselves to selective stratification we were able to determine all this and at the same time to secure vital groups of pottery without unduly exceeding our capacity for coping with them. I think we may fairly say that we made the best of both worlds and that our methods produced what *mattered*. But we had throughout a constant and painful struggle with our Pitt Riverine consciences.

WHAT *MATTERS* IN ARCHAEOLOGY ?

Well, there it is. I find myself, in this year of grace, praising the ravishers of Silchester and Mohenjo-daro and recommending *selective* stratification—all in the endeavour to get *what matters* out of a site at the expense of mere pedantry. After all, if we may dig selectively in the horizontal plane, we may surely dig selectively in the vertical plane. Dangerous advice. For who really knows what will *matter* as knowledge advances ? The whole basis of the work of General Pitt Rivers was that we do *not* know and must therefore record *everything*, however seemingly insignificant at the time of record. And Professor Hawkes's recent re-assessment of some of the General's evidence has shown us how right the principle was. But it was right *in the context in which the General was working* : on small sites representing relatively unevolved and reluctant cultures. There the archaeological bow-and-arrow was an appropriate weapon. Against an evolved and embattled civilization a more effective armoury is required : something short, indeed, of the blind bombing of a Schliemann, a St. John Hope or a Mackay, but something certainly including the use of high explosive. The major interrelationships of an elaborate society matter to us a very great deal ; indeed they are a primary concern to us ; and they will only yield to an all-out attack on a broad front. Later, there will be the mopping-up process, which can and should be carried out deliberately with careful small-scale work. In these minor operations British archaeology has in the past been at its best. The time has come now, I suggest, to go beyond this : to apply to big-scale operations the same skill and persistence which we have devoted to small ones. And this we can only do adequately if we remember that the big-scale operation is not merely the small-scale operation writ large. In important respects it is a different sort of manoeuvre. We proudly proclaim the Pitt Rivers tradition but do not let us make a fetish of it. Were the General alive today and confronted with the relics of a great central civilization instead of the smudges of his peripheral slum, he would, I have no doubt, echo fervently the words of Karl Marx and say ' At any rate I am not a Pitt Riversist '.

I appeal, therefore, for a higher measure of concentration, on the part of British archaeologists, upon the *major* achievements of Man as a social animal, and for the evolution of the necessary techniques for the purpose. It sometimes seems to me that we are constantly starting the race in fine form, but running out about half-way down the course. Let us for a change try to come in over the last jump. There is no sort of doubt that we in this country can supply the best initial field-training in the world. Grahame Clark's epoch-making work at Seamer, the annual forays of Ian Richmond and Eric Birley upon a long-suffering but still slightly resistant Hadrian's Wall, set a standard of scholarship scarcely approached by the best of our Continental contemporaries. Cyril Fox has revealed and lit up the mumbo-jumbo of Bronze Age burial with the fires of an unresting imagination. Must all this illumination be hidden perpetually in our ultimate mists ? I was standing not long ago upon a tumult of mighty mounds whither age after age came men from China, from the Mediterranean, from Ind, to exchange their goods and fertilize their ideas, to express the ancient world socially and aesthetically in the most complex polity then known to man. Could we but transfer a tithe of our scholarship for a few seasons to this great workshop of civilization—or indeed to many others of the same high potentiality—what should we not gain in our knowledge of human achievement ? What could *matter* to us more than that ?

For I am one of those elderly persons who still believe in that archaic phrase, the Nobility of Man. I do not close my eyes to Man-the-Jellyfish or Man-the-Whole-time Food-gatherer ; but I believe above all in Man-with-Time-to-think-between-Meals, in Civilized Man. Perhaps as an archaeologist I am merely taking the line of least resistance ; perhaps I am choosing, or attempting to choose, the kind of Man who had most to say and could express it most amply in material things and is therefore the most susceptible subject for the archaeologist. But I think not. I had already written these words when I heard Professor E. L. Woodward saying much the same thing, though in better shape. He was speaking of certain scholars who had regained for history a place among the Muses, affirming that they did so ' ultimately because they set a high value upon the dignity of man '. ' I repeat this term deliberately ', he added, ' because one of the signs of disintegration in our own culture is an unwillingness to consider that man has dignity, and that his acts may be noble. Once this conception of nobility is lost, history becomes nothing more than a rag-bag, a pawnbroker's catalogue, or at best a psychiatrist's case book '. I shall have a little more to add about that in conclusion, in a few moments. Meanwhile there is one more aspect of the matter which I offer for brief consideration.

I referred just now to Cyril Fox's reconstructions of burial-rites in Bronze Age Wales. Fox has recently assembled these reconstructions in the Chadwick memorial volume, and they make at the same time impressive and depressing reading. His account leaves us with the picture of the author we know so well as a man possessed of a trained imagination, of a kind which approximates to genius, pouring his soul into the revivification of the squalid memorials of creatures who had left civilization as far behind them as the Atlantic would (at that time) allow: creatures subsisting on the ultimate foothold of savagery. They scarcely deserve so much flattery; but it is not perhaps too much to say that Fox has almost created a new Fennimore Cooper tradition, that of the Noble (Bronze Age) Savage. He has at least set this savage upon his feet, even though but to bury him again. I remember how, many years ago when the sun was in the East, Fox and I were trudging across a desolate Welsh moorland and came upon a small barrow set within an earthen circle. Offa's Dyke came steadfastly up to the lip of the circle and then on the other side started off again with equal determination on a new alignment. The whole scene stirred Fox's ready enthusiasm, and a week or two later he had dug himself well into the landscape. The mound had by now vanished, and Fox stood, in the spirit, amongst its makers. He was almost physically present at the living ritual, the actual procedure of burial. I quote his own words in retrospect: ' Under the centre of the mound was a deep and large grave-pit on the floor of which lay the skeleton of a full-grown man. To enable this grave to be entered with ease and dignity a sloping passage from ground-level had been cut on the north side. Surrounding the grave-area was a circular trench, which also had a sloping entrance, and on the same side. *But it had no exit*: the area round the grave was isolated. The conclusions drawn from these facts were that the dead man's home was on the north side of the site chosen for his burial; that he had been ceremonially borne by friends or kinsfolk up to, and into, the trench: that those who carried him were not allowed to enter the consecrated area round the grave, but that the persons charged with the performance of the burial rites were awaiting the bearers beyond the trench. The body was handed over, and these persons descended with it into the grave '.

In the present context what matters to us in all this is not of course the particular episode but the creative act of imagination that has gone to the making or re-making of it. Individual after individual, learned society after learned society, year after year, we are prosaically revealing and cataloguing our discoveries, and are excessively content. We dig up mere *things*, forgetful that our proper aim is to dig up *people*. It is *they* that matter, not their pen-knives and their trouser-buttons. We justify our dehumanization of the past by attaching an almost fanatical virtue to what we call our *objectivity*. As so often we are thereby merely elevating our shortcomings into a principle. The only thing that really matters in our work is the *re-creation of the past*. For that we need something more, a great deal more, than a dog-Latin transcription of observed data. That is the anatomy of history; but the anatomy is meaningless without its vital interpretation. It is not enough that we archaeologists shall be a variety of natural scientists; we are, and don't let us forget it, above all, humanists. As I have said somewhere else, one sometimes detects today a tendency to devolve archaeology into a dehydrated humanism, to mummify the past, to transform our predecessors into 'battle-axe folk' or 'beaker-folk' until, by a further instinctive reaction, we begin almost to personify battle-axes or beakers with a sort of hungry latter-day animism. I will not, I trust, be misunderstood if I regret this tendency. However broadly we use the words, man is in some sense the casket of a soul as well as five-shillingsworth of chemicals. And his recorder must therefore be a good deal more than a rather superior laboratory-assistant. He must also be something of an artist. 'Historical understanding', recently remarked the Oxford historian whom I have previously quoted, 'is more than a series of detective tricks. It requires a mind already attuned to the scale of human action and practised in the subtlest use of language to express the depths and heights'. Those words of Professor Woodward's were well and timely spoken. The historian must have a spark of the comprehending imagination which inspires the painter or the poet. All great historians have this, and owe their greatness no less to it than to their scholarship. They make the past *live* because they are themselves alive and can integrate their reasoned facts with the illogicalities of life. A dangerous thing, undoubtedly, this integration of Reason with Rabelais; but without it we are mere cataloguers, adding dust to dust and ashes to ashes.

I have dwelt upon this matter, not because it is anything new, but because it is very relevant to certain present-day tendencies. After the war-interval I have again during the past few years come into contact with the younger generation of archaeologists, young people under thirty. And I have found in them a change which is due only in part to the impact of the war. Bluntly, as humanists or potential humanists, they are as a whole less well educated, less alert to matters of the imagination, than their predecessors. I am of course getting old, etc., etc., but I think, I fear, that what I say is true. 'Greats' and its equivalents are on the slippery slope. In ten years' time Greek will be to all intents and purposes dead, and Latin will be having a bad pain. These facts are significant pointers. What is replacing the traditional disciplines? A great variety of skills and techniques, but comparatively little of the kind that can educate and stimulate the historical imagination. For this vital, over-riding quality is, as I see it, only in part innate. It is liable in most of us to be a tender growth and needs careful nourishment. t needs the sort of nourishment that reflective literature of the highest quality, with a historical bias, can best supply. It needs something equivalent to a classical education, and I am not quite sure what the suitable equivalents are. Whatever they be, they are urgently necessary if we are to save archaeology from the technicians.

For the second time during these discursive remarks I listen to myself with a somewhat pained surprise. Throughout my life I have done my little best to emphasize the technical needs of archaeology. I even went so far as to found an Institute where these needs could in some measure be supplied. Perhaps again it is a symptom of old age hat, having tried to spur the horse to a gallop, I am now hanging on to the reins to prevent t from running away. Well, so be it.

And now lastly. I have pleaded for wider horizons, I have urged the importance of he comprehending imagination. But whither, at the best, can all this lead us? How ar can we hope to discover and recognize that which matters, as distinct from that which s merely new? What phases, what regions have really *mattered* in the story of human achievement ? The other day I rashly suggested to a Palestinian archaeologist that Palestine was rather a backwater. ' Yes ', he replied modestly, ' but, after all, we *did* produce the Trinity '. The conversation then lapsed. I have since been wondering, as every archaeologist has wondered sometime or other, what the value of our evidence really is. What do our bits and pieces amount to ? Listen, if you have courage, to the grave words which Dr John Donne uttered on this subject three and a half centuries ago :

> ' The ashes of an Oak in the Chimney are no epitaph of that Oak, to tell me how high or how large that was ; it tells me not what flocks it sheltered while it stood, nor what men it hurt when it fell. The dust of great persons' graves is speechless, too ; it says nothing, it distinguishes nothing. As soon the dust of a wretch whom thou wouldest not, as of a prince whom thou couldest not look upon will trouble thine eyes if the wind blow it thither ; and when a whirlewind hath blown the dust of the Churchyard into the Church, and the man sweeps out the dust of the Church into the Churchyard, who will undertake to sift those dusts and to pronounce, this is the Patrician, this is the noble flour, and this the yeomanly, this the Plebeian bran ' ?

Who indeed ? Not the poor archaeologist, who may at best bottle the dust and send it hopefully to Professor Zeuner. Dr Donne cannot be gainsaid, but see how he cuts at one of the roots of our discipline ! A Greek (was it Thucydides ?) somewhere remarked that a great nation may leave behind it a very poor rubbish-heap. And are we, as practising archaeologists, to award the palm to the unknown Sumerian who was buried at Ur with 63 helmeted soldiers, grooms and gold-garlanded damsels, two chariots and six bullocks, or to the Nazarene in a loin-cloth who was nailed up on Golgotha between two thieves ? I merely ask the question, but cannot help feeling that, were archaeology alone the arbiter, the answer would not be in doubt. Give us helmets and gold garlands every time ; bread and circuses give us, provided that the bread is carbonized and the circuses well-furnished with good solid bronze and marble. But let us at least, in our gratitude for these things, remember the missing values that cannot be appraised in inches or soil-samples or smudges in the earth.

All this is not very encouraging. The archaeologist may find the tub but altogether miss Diogenes. He must accept that risk, consoling himself with the reflection that no single approach to human accomplishment can be other than partial and chancy. The literary historian who overlooks art and craftsmanship and environment may lose as much as the archaeologist who can produce for us a harp without its music or a tub without its philosopher. Let us therefore count our blessings. We cannot read the language of the Minoans, but their palaces and frescoes, their wares and jewels, are themselves a pictographic language that tells us not a little of their way of living, and hints, however vaguely, at their way of thinking. We must do what we can with the material vouchsafed to us, in full consciousness of its incompleteness. But try we must, always with the thought that archaeology and history are alike frustrate unless they contribute to a vital *reconstruction of man's past achievement*, in other words aspire to interpretation as well as to mere transliteration. Sir Arthur Evans was a thousand times right in his interpretative attempt to reconstruct the Palace of Knossos, after faithfully recording the evidence as he found it. Mrs Hawkes and her colleagues were right in their attempt to reconstruct Little Woodbury and so, of course, was Cyril Fox in his reconstruction of a British chariot. The task of reconstruction, whether three-dimensional or two-dimensional, is one from which the archaeologist must not shrink. It is the crown of his work. And it is surprising and reassuring to find how much good constructive material can in fact be extracted from a rubbish-pit—or lie implicit, for that matter, in the tale of a tub.

Basilica Discoperta

by A. M. Schneider

THE question of origin and pedigree of the Christian basilica has occupied scholars again and again, but no generally recognized result has been achieved so far (1). Recently Ejnar Dyggve has put forward the thesis that the old Christian basilica is the final result of a long development which started with the classical Heroon. Dyggve himself has excavated such a Heroon at Kalydon and has published a fine report on it (2). It is the tomb and sanctuary of the chief of a clan, Leon, who was regarded by his descendants as a new Heracles. The building consists of a peristyle with a transept added in front of the north side which opens into a square sanctuary (FIG. 1). In the sanctuary is an altar and at the rear wall a base on which stood presumably the statue of the deified dead. The sanctuary was closed in front by a balustrade. Underneath it is a room serving as tomb with two beds of stone ; this room was accessible from outside. About the same time a very similar building was discovered on the citadel of Pergamon (FIG. 2), the only difference being that there was no burial vault underneath the sanctuary, but only a shaft reaching down to the virgin soil which probably once contained funerary urns. The excavators (3) take it to be a monument dedicated to the cult of the rulers of Pergamon—which means that we have here again a Heroon. These two similar structures nvite the guess that we have to deal here with a widespread type and this interpretation s corroborated by another building in Kalaureia (4). Dyggve thinks that this type of structure is the prototype of the Christian basilica of the martyrs and not only of this but of the Christian basilica altogether, which consists also of a nave with rows of columns with sometimes a transept to which is joined the apse with altar and *confessio* of the martyr. This derivation seems to be confirmed by the discovery of another Christian sanctuary at Marusinac near Salona which forms, so to speak, the connecting link. There Dyggve (5) found a vaulted mausoleum with an altar under which was a burial vault with the coffin of Anastasius who was executed in A.D. 304. In the course of the 4th century a Christian cemetery grew up round this martyrion which was destroyed in 395 and then overbuilt with a bigger structure (FIG. 3). East of the martyrion is a sepulchral basilica of the usual shape. The relics of Anastasius were translated into this building and buried in a small *confessio*. North of it is a second building consisting of an apse, two transepts of the type of the *liwan*, and a colonnaded open court with tessellated floor. n the roofed-in parts are tombs of bishops and other persons of social standing. We have therefore here a type of building somewhat similar to the pagan Heroon. Dyggve thinks that this type of building represents the *basilica discoperta*. He takes this term from an anonymous pilgrim of Piacenza who visited the burial temenos of the

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[1] I mention only the relevant publications and papers by Zestermann, K. Lange, F. Wirting, Le Roux, Bréhier and R. Schultze.

[2] E. Dyggve—F. Poulsen—K. Rhomaios, Das Heroon von Kalydon. Kopenhagen 1934.

[3] E. Boehringer—F. Krauss, Das Temenos fuer den Herrscherkult. Altertuemer von Pergamon 9 (1937).

[4] G. Welter, Troizen und Kalaureia, Berlin 1941, 51 and plate 44.

[5] Forschungen in Salona III. Wien 1939. E. Dyggve and R. Egger, Der altchristliche Friedhof Marusinac. Dyggve's views with regard to the Basilica Discoperta are elaborated in *Atti del IV Congresso Internazionale di Archeologia Cristiana I.* Roma 1940, 391 ff. and in *Zeitschrift fuer Kirchengeschichte* 59, 1940, 103 ff.

Old Testament patriarchs at Hebron towards the end of the 6th century and left us a description of it. The evidence for reasoning that the basilica of the martyrs of the time of Constantine is descended from the Heroon of classical times seems to be complete.

On the other hand, Theodor Klauser (6) has already pointed out that an apse occurs also in other connections than in rituals connected with burials. He has argued further that the transept is only loosely connected with the peristyle court, and that the transept of the Heroon does not offer a convincing analogy to the transept of the basilica. His main argument against Dyggve's thesis is the fact that the oldest basilica with transept and apse of the time of Constantine, the Lateran basilica built 313, was a church for regular assembly *without* a martyr's tomb. As far as we know, no bigger basilica intended for the cult of martyrs had preceded it which could have served as model for the Lateran basilica. Furthermore, the custom of depositing relics in the altar grew up only in the course of the 4th century. Klauser is inclined to admit that the architect of the Lateran basilica of the time of Constantine had peristyle courts in mind when he designed the

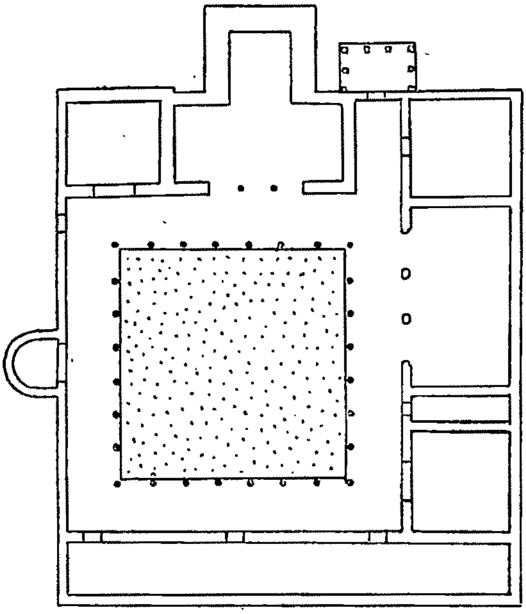

FIG. 1. KALYDON, HEROON. Scale 1:565

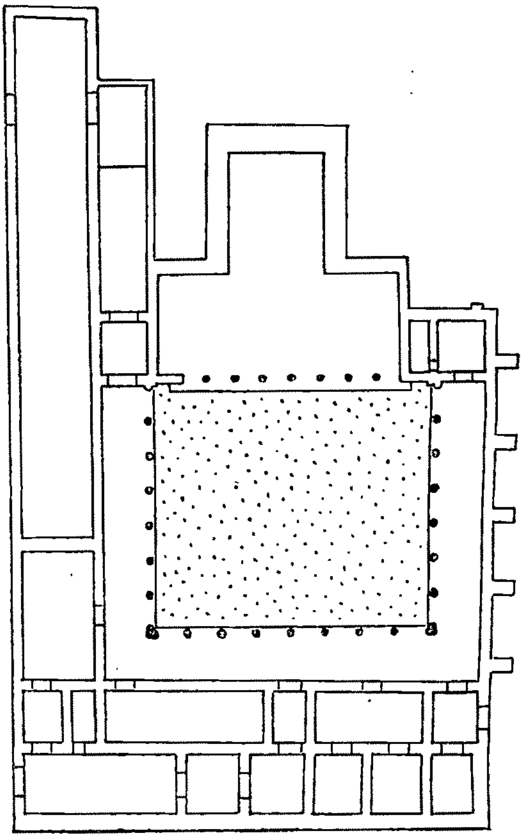

FIG. 2. PERGAMON, HEROON. Scale 1:545

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[6] Vom Heroon zur Maertyrerbasilika. Bonn 1942. Kriegsvortraege der Rhein. Friedrich-Wilhelm Universitaet Bonn a.Rh. Heft 62.

ave. I think Klauser is right in his objections and some others may even be added to em. A priori we have to admit that Dyggve stresses rightly the importance of the cult f the dead among Pagans and Christians and especially of the cult of the martyrs among e christians. But the cult of the dead and of the martyrs takes place outside the cities nd is celebrated only once a year on the anniversary of their death. The ordinary ivine service has nothing to do with it; it is performed inside the city boundaries

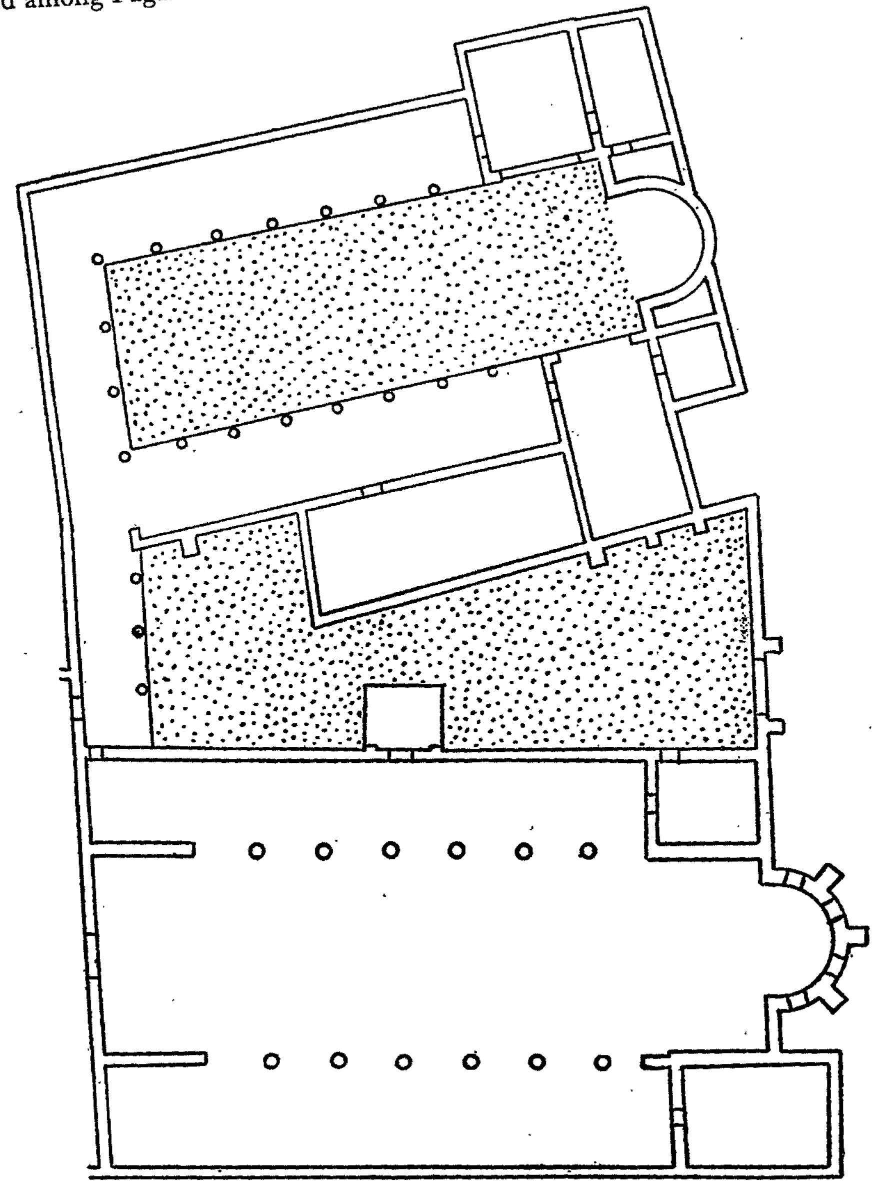

Fig. 3. MARUSINAC. Scale 1:455

either in private houses or, after the 3rd century, in specially designed buildings (**7**), the churches. The eucharist was celebrated from the earliest times (Acts, xx, 8 ff.) in closed rooms (7a) whose entrances were carefully guarded in order that no profane eye should observe. In classical times the mystery rites prefer also closed rooms, whereas other religious ceremonies take place under the open sky. This is not compatible with the assumption that the open peristyle court was the precursor of the Christian basilica, which developed according to the needs of the liturgy of the congregation. Archaeology is not yet able to show all the details of this development, but literary sources and the meagre archaeological evidence provide at least a rough outline. The pattern of the liturgy was already established by about the middle of the 2nd century: it consisted of a service with the reading of a gospel and epistle, and a sermon, followed by the eucharist proper from which the catechumen were excluded (**8**). The congregation consisted of the laity (strictly separated according to age and sex) and of the clergy who were again separated from the laity and, in medium-sized communities, comprised bishop, presbyters and deacons (**9**). The laity not only responds to the reading but takes an active part in the ceremony of the eucharist by carrying offerings to the altar from which those items are chosen which are necessary to the liturgy (**10**). During the communion they receive then what they have brought to the altar as prosphora. Therefore a separate room was needed for the altar with the clergy assembled round it, but also sufficient room for the two processions of the laity to the altar. The oldest known private church in a house at Doura-Europos (**11**) shows at least that the nave was orientated: at the east wall is a slab either for the bishop's seat or for the altar. This arrangement initiates a development as we find it in the basilica of Aquileia built in 313 (**12**). Here the arrangement of the tessellated floor proves without doubt that we have a transept with altar for presbyters and a three-naved room for the laity. The different panels of the mosaic are presumably intended for the accommodation of the different groups of the laity, separated according to age and sex. In a room like this the ritual of the liturgy as described could be performed according to the underlying idea. Such a room fits the requirements of the cult and has developed without doubt from customary practice. We also know of cult buildings of basilican shape with three naves during classical times. In this

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[7] Ecclesia—Churchbuilding in Tertullian and Cyprian cf. H. Janssen, Kultur und Sprache 1938 (Latinitas christianorum primaeva 8) pp. 24 ff.

[7a] The celebration of mass in the open air even today requires the special permission of the bishop.

[8] cf. Justinus Martyr, Apologia 1, 65–7.

[9] The *Didascalia Apostolorum* of the middle of the 3rd century states in this connexion cap. 12 (ed. P. de Lagarde, Goettingen 1911, 56): 'There shall be a separate space for the priests on the east side of the house and the throne of the bishop shall be put up between them; the priests shall sit with him. And on another side of the eastern part shall sit the men of the laity. For it is appropriate that the priests sit with the bishop in the eastern part of the house, then the men of the laity and then the women, so that when you stand to pray, the rulers may stand first'.

[10] According to the *Didascalia* 'one of the deacons should always be present at the Prosphora of the eucharist'. This rule only makes sense if he had to regulate the offerings of the believers because he did not need to keep watch on the korban (offering). For Africa this offering is vouched for by Tertullian, Apologia 39; Cyprian, Epist. 28; de opere et eleemosynis 15. The relevant quotations in the Sacramentarium Leonianum have been collected by Klauser: *Roem. Quartalschrift* 43, 184 ff.

[11] C. Hopkins, Christian Church at Doura-Europos, Preliminary Report of fifth season of work of the Excavations at Doura-Europos. New Haven 1934, plate 39.

[12] cf. A. Gnirs, *Oesterreich. Jahreshefte* 19/20, 1919, Beiblatt 194, fig. 88.

connection should be mentioned the building for a mystery cult near the Porta Maggiore in Rome (13) and the building for the cult of Serapis at Miletus (14). As we know hardly anything about Pagan liturgies we can only guess that some of them may have resembled the Christian liturgy. This appears to have been recognized also by the Christians, who ascribed this similarity to the influence of evil demons (cf. Justin, Apologia I, 58, 4). Similar liturgical usage produced a similar room, and it is even not necessary to deduce a reciprocal dependence. The Christian cult room is therefore to be regarded as originating solely from the facts of the cult, while it is obvious that for the monumental elaboration of the basilica of the time of Constantine the classical market basilica, and not the peristyle court, provided the inspiration. The market basilica, on the other hand, is a room *sui generis* which had to serve other purposes and was therefore subject to other principles of planning : it is without orientation or at least neutral with regard to orientation. This meant that it opened towards the long sides or towards the short sides according to its local position. But the vital centre of the market basilica is always the middle, surrounded by columns on all sides. When the architects of Constantinian time built the great representative basilicae, they were tied to the already developed scheme which was in plan close to the *basilica forensis* ; on the other hand it was the *basilica forensis* which provided them with the necessary means for the architectural elaboration. Therefore it is out of the question that peristyle courts have served as prototypes and it is inconceivable why the Constantinian architects should have taken that roundabout way, especially as open court and roofed-in hall are independent elements of architecture which have always existed side by side and which have not developed one from the other.

Eusebius (15) terms the church building *ἐκκλησία, οἶκος ἐκκλησίας, οἶκος εὐκτήριος* and also *βασιλική* (Vita Const. 3, 52 ; it has to be noted that the Vita was apparently re-edited and revised in later times), but in *Hist. Eccl.* 10, 4, 42 *βασιλικὸς οἶκος*. This last term proves that the author was not quite familiar with the substantive *βασιλική*. This is not surprising as the basilica was not a Greek but an Italian invention. In Greek *βασιλική* is always used as an adjective (*βασιλικὴ στοά, βασίλειος στοά*) ; it occurs as a substantive only where it is a translation from basilica (16). The Greek knows neither the architectural type—in the Greek market stood the Buleuterion and not the basilica—nor the name. The *βασίλειος στοά* in the market of Athens which has been regarded for a long time as the prototype of the basilica looks quite different (17), and must be eliminated as model. Basilica as a substantive is known only in the Latin language, in fact for the oldest known building of this type, the basilica Porcia (about 184 B.C.). Of the older basilicae known from literary evidence, the only one still preserved is the basilica Aemilia which was from the very beginning three-naved as well as roofed-in. If we look for prototypes of the Christian basilica we have to turn, if at all, to the roofed-in market basilica and not to the peristyle court. The peculiar *basilica discoperta* of Marusinac ought therefore to be disregarded in this discussion. But we have to put the question : Where does Marusinac come in, and how is it to be explained ? It must be stated at once that *basilica discoperta* is not a technical term. It is found only once in the entire early

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[13] M. Carcopino, La Basilique Pythagoricienne de la Porte Majeure, Paris 1927.

[14] Milet, I, 7 : H. Knackfuss, Der Suedmarkt. Berlin 1924, 180.

[15] cf. P. Mickley, Die Konstantinkirchen im hl. Lande (Das Land der Bibel IV, 3). Leipzig 1923, 30 ff.

[16] First mentioned as far as I know in an inscription in Oropos in A.D. 73 ; Dittenberger, Sylloge Inscr.Graec^3. Nr. 747.

[17] Groundplan *Hesperia* VI, 1937 ; plate II.

Christian literature, i.e. in the already mentioned Anonymus Placentinus who travelled in Palestine in 570 and left a description more naïve than lucid (**18**). In Hebron the pilgrim found the temenos built by Herod I above the tombs of the patriarchs, a walled-in square of 60 by 33 m., the walls of which are still standing today to a height of about 10 m. Of this classical type of architecture the simple pilgrim had no more idea than had the Anonymus of Petrus Diaconus who described it as *ecclesia sine tecto* (**19**). On the other hand the man from Piacenza described it in chapt. 30 as follows : *in quo loco . . . basilica aedificata est in quadriporticus, in medio atrio discopertus* (variant : *atrium in medio discoopertum*) *per medio discurrit cancellus, et ex uno latere intrant Christiani et ex alio latere Judaei, incensa facientes multa.* The approximately contemporary recensio B, reads as follows (**20**) : *Est basilica in quadriporticus, atrium in medio discoopertum et in*

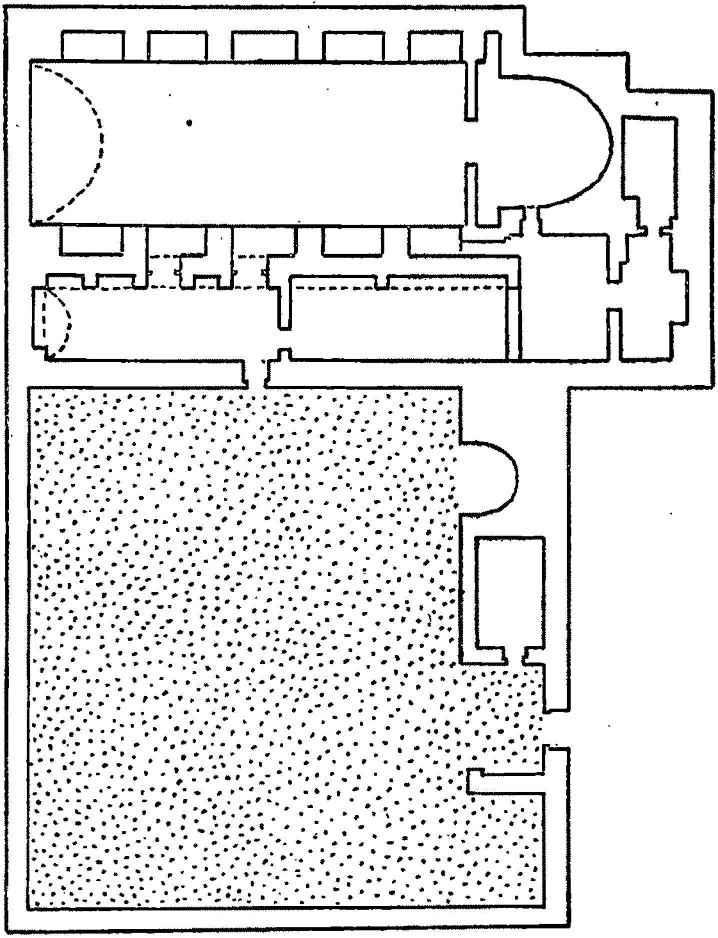

Fig. 4. KEFR ZEH, MAR AZIZAEL. Scale 1 : 243

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[18] P. Geyer, Itinera Hierosolymitana C.S.E.L. 39, 1898, 179 and 209 ; the attribution of this report to Antoninus cannot be maintained ; cf. Grisar, *Zeitschr. f. kathol. Theologie* 26, 1902, 760 f.

[19] Geyer, l.c. 110. [20] Geyer, l.c. 209.

medio cancellatum . . . (variant: *basilica aedificata est in medio atrio quadriporticus discooperta*). As both versions with their variants prove, no one was able to get a clear idea of the facts from the description. Either the pilgrim saw only four *porticus* running parallel to the enclosing walls or, as recensio B seems to suggest, a basilica with peristyle.

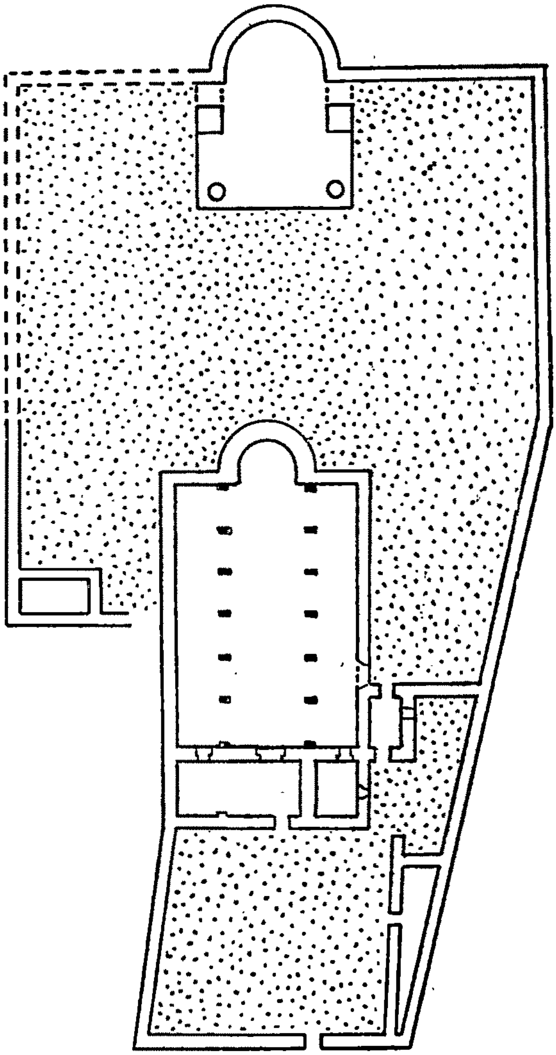

FIG. 5. BIN BIR KILISE No. 7. Scale 1:555

What was in fact there cannot now be decided on the spot. If the latest views of those dealing with the problem (21) are correct, namely that the layout of the crusader church, today a mosque, inside the temenos points to an early Christian basilica, then we should be dealing with a structure such as recensio B seems to imply. In any case it cannot be interpreted as a building like Marusinac nor can Marusinac be regarded as basilica

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[21] H. Vincent—E. Mackay—F. Abel, Hébron, Le Haram el-Khalil, Paris 1923.

discoperta. Such a type of building never existed, whatever interpretation the clumsy text of the pilgrim may suggest. The structure in Dalmatia has to be explained in a different way. Open places of worship surrounded only by a wall seem to have been used in the Jewish cult, i.e. the Proseuche (22). It is possible that the islamic musalla (the place where the *salāt* are performed) belongs to this context, an enclosed open square with a mark giving the direction for the prayers (23). It is used for prayers for the dead, also for prayers for rain. These open praying places are found also in the Christian world, and there is even a name for them known from the Tûr 'Abdîn, i.e. *bēt selôtā* : place of worship. They have been identified in Kefr Zeh (FIG. 4), Hashtarak, Hah, Arnas and Bassibrina (24). They show an open square south of the generally single-naved church with an apse towards the east which is known according to a Syrian inscription on the building as *bēt selôtā* (25). The question now is—what liturgical functions took place there ? Miss Gertrude Bell elicited the information that the service was performed there on weekdays (26). This cannot have been the original purpose of the building, as Pognon relates (l.c. 42 ; 93, note 1) that the monks were having the daily office there during the hot season (27). In fact there is no altar in the *bēt selôtā*, but a *gûdā* (pulpit) on which the books for the prayers of the hours are laid (28). Such stone-built pulpits have their place also in the church immediately in front of the choir : here the prayers of the hours are said when the weather is bad. Where there is no *bēt selôtā* the office is held in front of the entrance of the church during summer where again such pulpits are found. However, this also does not seem to have been the sole and original purpose of this remarkable structure. In the *bēt selôtā* conche of Hah and Hashtarak there are many commemorative inscriptions in honour of the bishops and monks buried nearby in a common burial vault. The epitaphs of Hah date from 1136 to 1296 (Pognon Nr. 65–71), those from Hashtarak from 913–1294 (Pognon Nr. 95–116)—the structure points therefore unequivocally to the cult of the dead. It is probable that the daily office was performed also with reference to the dead brethren (28a). If we turn

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[22] cf. Zarb, De Iudaeorum προσευχή Angelicum 5, 1928, 91 ff.

[23] Handwoerterbuch des Islam, Leiden 1941, 549 ; I saw such places of prayer at Brussa, Iznik (near the Lefke-gate) and in Constantinople, but they are rare or have been built over in later times.

[24] cf. Miss Bell in : M. van Berchem—J. Strzygowski, Amida. Heidelberg 1910, 243, fig. 174 and 175 (Kefr Zeh) ; 249 fig. 184 (Arnas) ; 255 fig. 195 (Hah, Mar Sovo) ; cf. also Monneret de Villard, Chiese della Mesopotamia, Roma 1940. *Orientalia christ. Analecta* 128, 48. The bēt selôtā of Hashtarak was built before 772, the others are younger, but apparently reconstructed as the church of Kefr Zeh belongs to the early 7th century.

[25] H. Pognon, Inscriptions semitiques des la Syrie, de la Mésopotamie et de la Région de Mossoul. Paris 1907, 93, Nr. 51 (Kefr Zeh).

[26] Amida l.c. 245. It is to be noted, however, that the Nestorians did not celebrate any liturgy in the early days during the ferial days, but kept only to the hourly prayers (teshmeshta) : R. H. Conolly, Anonymi auctoris Expositio officiorum ecclesiae 2, 4 and 3, 4 : *Corp. Script. orient. Script. Syri, Series* II, 91 ; Versio, pag. 107, 175.

[27] With this would correspond the fact that the Talmud knows summer and winter synagogues : b. baba bathra fol. 3b (Goldschmidt 925) and that in Constantinople there exist also summer and winter mosques. The mosque of Mimar Sinan in the valley of Lycus, for instance, shows two buildings side by side ; today only their foundation walls are preserved.

[28] cf. Pognon l.c. 43.

[28a] This would fit well with the fact that in Salah (Mar Yakub), though there is no *bēt selôtā*, the memorial inscriptions are existing in the entrance hall (dated from 908 to 1370 ; Pognon l.c. 62 Nr. 22–33), where also the daily officium took place during the summer.

Now again to the Dalmatian building which was also devoted to the cult of the dead, it is clear that this is not accidental. The monks of the 10th century were carrying on with a custom of early Christian time of which the oldest preserved example still in existence is Marusinac. In this connexion it is to be emphasized—and this seems to me very important—that precisely in Salona we know of Christians hailing from Syria (**29**). The assumption is permissible that in the peristyle court at Marusinac (as far as it was covered with mosaics) meals in honour of the dead took place, especially for the benefit of the poor (**30**). The court chapels at Korykos (**31**) and Bin bir Kilise (FIG. 5) (**32**) which are comparatively near to the Syrian and North Mesopotamian area served probably for the same purpose. The so-called *basilica discoperta* has developed from the perhaps local practice (**33**) of the cult of the dead and is a special case which cannot be connected with the regular divine service and which, therefore, cannot be worked into the pedigree of the Christian basilica (**34**). It must remain undecided whether the open place of worship, at home in the Orient and only there justified, was influenced by the Hellenistic Heroon. I do not think it very probable.

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[29] Salona III, 114.

[30] cf. the Commentary on Job, Migne P.G. 17, 517: *Celebramus nimirum religiosos cum sacerdotibus convocantes . . . invitantes adhuc egenos et pauperes, pupillos et viduas saturantes, ut fiat festivitas nostra in memoriam requiei defunctis animabus, quarum memoriam celebramus.*

[31] G. Bell, *Revue archéologique* 1906, II, 8, fig. 1; 28, fig. 20; Olbia 34, fig. 26.

[32] Fig. from K. Liesenberg, Der Einfluss der Liturgie auf die fruehchristliche Basilika. Neustadt 1928, 149, fig. 58. Kirche VII.

[33] Perhaps the building at Marusinac can be connected with the curious twin buildings at Salona and Aquileia. Besides the basilica with which I have dealt above stands a second one but without altar—is this second basilica possibly a room for the cult of the dead? Or a room for agape? It is possible that the custom of the meal in honour of the dead was also known in Mesopotamia. In the Vita of Mar Daniel Asya (Bedjan, Acta Martyrum et Sanctorum 3,505) we find the description of a Nestorian church with 'three spacious courts and inner cells for schools and Vigil celebrations'. The word shahra can mean Vigil as well as cena funeraticia.

[34] A. Boethius, Festkrift tillägnad J. Arvid Hedwall 1948, 71 f. follows up Dyggve's thesis and quotes as further examples the basilica of S. Sebastiano in Rome as well as the structure in front of Sta. Costanza. But I cannot see how he can prove the case for S. Sebastiano. Sta. Costanza has to be left out too, as the outer wall shows many big windows which would be quite unnecessary if it had been a court basilica. I am also very sceptical when Dyggve thinks that the hypaetral basilica can be identified in palaces of late classical time (Ravennatum Palatium sacrum. Kopenhagen 1941, 10; 48; 53 *Kgl. Danske Videnskab. Selskab. Arch.-kunsthist. Meddelelser* III, 2). I find it difficult to believe that the area outside the entrance to the palace at Spalato can be regarded as a ceremonial hall. Diocletian built and occupied the palace as privatus when he gave no more receptions in his quality as emperor. I see in the arcades no more than relics of the colonnades along a street which lack the accompanying wall. This wall had to be omitted because, if it had existed, it would not have been possible to see the temple and the mausoleum. The Chalke in Constantinople may have been copied by Theodoric, but we have no witnesses that ceremonial receptions took place there; neither does it seem necessary to reconstruct the building at Ravenna as done by Dyggve on plate XII. I see no reason not to assume that the façade as represented in the mosaic of San Apollinare Nuovo stood with a straight line colonnade. Such straight frontal colonnades from which projects the propylon are known; I mention only the façade of the pre-Justinian church of Sta. Sophia at Constantinople which I excavated and the central hall of the camp of Diocletian at Palmyra (Wiegand, Palmyra 1932, Textband 94, fig. 106). In Ravenna we would have correspondingly the front of the palace precinct. I have to add that the arcaded street leading to the palace in Constantinople was two-storied: Cod. Theod, 15, 1, 45.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

CHINA'S DISCOVERY OF AFRICA, by J. J. L. DUYVENDAK. Probsthain, London, 1949. 35 pages. 6*s*.

THE BUILDING OF HADRIAN'S WALL, by C. E. STEVENS. *Soc. of Antiquaries*, Newcastle-upon-Tyne, 1948. 3*s*.

THE CENTENARY PILGRIMAGE OF HADRIAN'S WALL, 4TH–9TH JULY, 1949, compiled by ERIC BIRLEY. Same as last, 5*s*. [Excellent bibliography].

THE NORSE STYLE OF ORNAMENTATION IN THE VIKING SETTLEMENTS, by HAAKON SHETELIG. *Acta Archaeologica*, XIX, 1948, 69–113.

MAN THE TOOL-MAKER, by KENNETH P. OAKLEY. Brit. Mus. (Nat. Hist.), 1949, 2*s* 6*d*. [An admirable popular account, giving a full account, with drawings, of types of stone implements and how they were made].

NEW EVIDENCE OF THE ANTIQUITY OF PILTDOWN MAN, by KENNETH P. OAKLEY *and* C. RANDALL HOSKINS. *Nature*, 11 March 1950, p. 379. [Derived from fluorine analysis].

RICHBOROUGH, 4TH REPORT, by J. P. BUSHE-FOX, C.B.E. Research Reports, *Soc. of Antiquaries*, 1949. Quaritch, £2 2*s* 0*d*. [A further instalment of this now classic series needing no commendation here].

BRITISH PREHISTORY, by STUART PIGGOTT. Home University Library, Oxford, 1949, 5*s*. [An excellent popular account based on much original work].

A HUNDRED YEARS OF ARCHAEOLOGY, by GLYN DANIEL. Duckworth, 1950. 21*s*. [A much needed history covering new ground].

32 BERICHT DER RÖMISCH-GERMANISCHEN KOMMISSION, 1942. De Gruyter, Berlin, 1944 (published 1950). [We heartily welcome the reappearance in excellent format of this celebrated publication].

WILLIAM STUKELEY, AN 18TH CENT. ANTIQUARY, by STUART PIGGOTT, O.U.P. 18*s*. [More than a biography of S. and very good reading].

BRITISH ANTIQUITY, by T. D. KENDRICK. Methuen, 21*s*. [A history of antiquarianism in the pre-Stukeley period and equally readable].

THE EARLY CULTURES OF NORTHWEST EUROPE: 22 essays written in honour of H. M. CHADWICK, edited by SIR CYRIL FOX and BRUCE DICKINS, C.U.P. £3 3*s*. [A valuable and scholarly festschrift].

THE MOA-HUNTER PERIOD OF MAORI CULTURE, by ROGER DUFF. Whitcombe and Tombs. 3–4 Addle Hill, Carter Lane, E.C. 4. £1 12*s*.

Correspondence

Dear Sir,

We have read with interest the article in your June issue, by J. F. Ewing, entitled ‘ A new technique for removing bones from limestone breccia ’.

For several years we have been investigating the possibilities of the development of fossils from rocks of all geological ages by chemical methods, and feel that some of our findings may be of interest to Father Ewing and to others among your readers. These results are being published from time to time in the ‘ Museums Journal ’ (*Mus. Journ.*, 48, p. 54; 49, p. 293), but perhaps we may mention here some points in which our technique differs from that of your contributor.

It has been found that the most satisfactory solutions for removing calcareous matrices from bone are a 20 per cent (by volume) solution of acetic acid, or one of 10 per cent Formic acid. In special cases weaker solutions are used. Acetic acid is cheaper and kinder to the skin than formic acid and is thus in more general use. A mixture of these acids attacks bone and is therefore useless. It has not been found necessary to add any other substance to these solutions and good results have been obtained with a very wide range of specimens, from Devonian Ostracoderms to Neolithic bone and ivory implements and ornaments.

In no instance have we been able to detect damage to bone attributable to chemical action of the acetic acid, but where exposed cancellous bone is infilled with calcite the violent eruption of carbon dioxide may damage the delicate bony tissue. It is then desirable to ‘ screen ’ these areas of bone from the action of the acid and polystyrene has been found to be the most effective acid-proof substance to employ. It is applied as a dilute solution in ethyl acetate. Several coats are put on, each being allowed to dry before the next is added. Any excess may be removed subsequently with benzine. The total drying-time is much less than that mentioned in Father Ewing's article for the chlorinated rubber solution.

For comminuted or decayed bones Plenderleith's method of impregnation has been found very effective (H. J. Plenderleith, 1934, ‘ The preservation of antiquities ’, p. 19, *Mus. Assoc., London*). The specimen is impregnated with a dilute solution of celluloid before immersion in acid and the thin film of celluloid left will be sufficiently strong to hold the bone fragments together when the matrix is removed, but because of its permeable nature will not impede the action of the acid. The insoluble part of the matrix will remain attached to the bone, but can be removed easily by brushing the surface with a camel-hair brush dipped in acetone.

After all the matrix has been removed the specimens are washed in running water for several hours. They are then allowed to drain, but not to dry. The next stage is to immerse them in an aqueous emulsion of polyvinyl acetate. The strength required is about one third of that supplied by the makers. This impregnation may be done *in vacuo* (*Mus. Journ.*, 49, p. 293), but where vacuum equipment is not available an immersion of from six to twelve hours will ensure adequate penetration. On removal from the emulsion the specimens are placed on a rack to dry. A suitable rack can be made from strips of zinc sheet, on edge : care must be taken to ensure that the bone is in contact with as few edges as is consistent with safety, and that these are oiled to prevent sticking. This treatment ensures that the bone will not break up on drying. The emulsion is transparent when dry, preserves the natural tints of the specimen and does not obscure detail.

Trusting that these observations may be of use to your readers.

We are, Yours faithfully,

British Museum (Natural History). H. A. Toombs and A. E. Rixon.

Notes and News

PESTILENCES

'THE DATE OF CAMLANN'—AND OF THE PESTILENCE OF THE SAME YEAR

I have read the note by Mr P. K. Johnstone, entitled 'The Date of Camlann', in the March number of ANTIQUITY (p. 44) with great interest.

The article interests me primarily as a student of the epidemics which ravaged these islands during the 6th century, but the possibility—brought out by Mr Johnstone—of Arthurian chronology also being concerned and possibly clarified adds to the interest of the problem.

There are two entries in the *Annales Cambriae* which are involved, namely:

> '537. XCIII Annus. Gueith cam lann, inqua arthur and medraut corruerunt: et mortalitas in brittannia et in hibernia fuit', and
>
> '547. CIII Annus. Mortalitas magna inqua pausat mailcun rex genedotae'.

Mr Johnstone's final misgiving 'We cannot be sure that the compiler of the *Annales* did not blunder in assembling his entries from disparate sources', though true, seems to me to invalidate the whole of his argument—and, indeed, to render the whole of this enquiry of no moment.

Possibly the most pertinent question is the reliability of the dating of the *Annales Cambriae*. The editor of the *Annales* in the Rolls Series, John Williams ab Ithel, describes the three manuscripts (A, B and C): of these, A. (Harley 3859) is the oldest, and is 'of the latter part of the tenth or the beginning of the 11th century' (p. x.)*

The years in the *Annales* are written down to 977 though the last event recorded is in 954, which points to their being finished *c.* 954 or 955, according to Phillimore.

'The chronology of this document', says Ab Ithel 'is designated by the repetition of the word "annus" for each successive year, whether blank or otherwise, whilst every tenth year is marked x, xx, etc. . . . From a comparison of the dates assigned to many of the events noticed in it by other writers, it would appear that the era on which its chronology rests would concur with the year' [445] 'of the Incarnation, though there is no reason given for this particular date'. (p. XXIV). He has compiled his marginal dating of the entries by adding an extra year for each annus mentioned, from 445 [=year 1] onwards, and correlating with Christian dating. A glance at the facsimile from the manuscript which is given as a frontispiece to the volume, shows at once how the dating has been arrived at.

I can see no obvious flaw if known dates such as the death of Offa in 796 ('Annus CCLII' plus the previous 444 years of the Christian era) be taken as test cases. On the other hand the two dates 537 and 547 do not, as the following argument shows, seem to be correct for the outbreaks of pestilence they chronicle.

Justinian's plague was raging in Constantinople in 542, and appears to have spread north-westwards across Europe. We know from Gregory of Tours (*History of the Franks*, IV, 5) that various parts of France were suffering from it in 543: the plague, called *blefed*, reached Ireland, according to the *Annals of Ulster*, in 544 (really in 545, since these annals are one year behind the true date at this period). If it was carried to these islands from Armorica by means of Celtic saints, it is possible that it may have arrived in Wales and spread to Ireland or the opposite may have happened. There is no record of either pestilence having spread from Wales into England also. The *blefed* must be distinguished from the Yellow Plague which followed it, according to the *Annals of Ulster*, in 548 (really in 549) and which doubtless overlapped it.

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* Early 12th century, according to Phillimore.

The following table shows the sequence of the outbreaks of the two pestilences, with the recrudescences of the Yellow Plague :—

DATE	PESTILENCE	SOURCE OF INFORMATION
[537.	' Mortalitas ' in Britain and Ireland	*Annales Cambriae*].
545.	'*Blefed*'. (' Mortalitas prima quae dicitur blefed ', *Annals of Ulster*: ' Mortalitas magna ', *Annals of Tigernach*).	*Annals of Ulster* (under 544), *Annals of the Four Masters* (under 543), *Annals of Tigernach* (539 or 540), *Chronicon Scotorum* (under 541), *Annals of Clonmacnoise* (under 545).
[547.	' Mortalitas magna '.	*Annales Cambriae*].
549.	' Mortalitas magna ' in Ireland (the Yellow Plague, *cron chonaill*).	*Annals of Ulster* and *Annals of the Four Masters*.
550.	' Boy Conell ' begins in Ireland.	*Annals of Clonmacnoise*
551.	' Mortalitas magna, i.e. Crom Conaill '.	*Annals of Tigernach*, *Chronicon Scotorum*, *Annals of Inisfallen* (under 541).
556.	' Magna mortalitas '.	*Annals of Ulster* (under 555).

Mr Johnstone says, ' No other pestilence is mentioned between 500 and 539. The Blefed must be the pestilence mentioned by the *Annales Cambriae* along with Camlann ', but as I have just shown, both of the *Annales Cambriae* entries (included in the above table) are earlier than the corresponding Irish entries in the *Annals of Ulster*—the 537 one by seven (or eight) years and the 547 one by one year (or two years). W. BONSER.

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PESTILENCES IN SIXTH CENTURY BRITAIN

Mr P. K. Johnstone's note in ANTIQUITY, XXIV (1950), 44 is valuable not only for the light it throws on the date of Camlann, but for the distinction between two pestilences afflicting Britain in the mid- 6th century A.D. Such plagues can be of considerable chronological importance, and, in the 5th century, Mr C. E. Stevens has drawn attention to the probable equation of the *pestifera lues* or *famosa pestis* placed by Gildas shortly before the Saxon invasions with the *pestilentia quae fere in toto orbe diffusa est* recorded by Hydatius in relation to a comet datable to 442 (1). Incidentally, it appears that this plague is independently referred to in the *Vita Sancti Winwaloei*, written by Wrdisten, Abbot of Landevennec, in the second half of the 9th century, as that which drove Fracanus, cousin of Catovius the ruler of *Nomnia* in Britain (emended by Doble to *Domnonia*) to Brittany some time in the 5th century (2), though Wrdisten may have simply elaborated his story from the mention of plague in Gildas.

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[1] *Eng. Hist. Review*, LVI (1941), 363.

[2] A. de la Borderie, *Cartulaire de l'Abbaye de Landevennec* (Rennes, 1888) ; G. H. Doble, *Saint Winwaloe* (Cornish Saints, no. 4 ; 2nd Edn., 1940) ; Kenney, *Sources of Early Irish History, I*, p. 175.

The recent discussion of the medical aspects of the pre-Conquest plagues of Britain by Dr Bonser and Lt.-Gen. Sir William MacArthur (3) gives point to Mr Johnstone's separation of the '*blefed*' of 539–540 from the 'Yellow Plague' of *c.* 549, the most famous victim of which was Maelgwn Gwynedd. Sir William points out that the explicit descriptions of the symptoms of the *flava pestis* preclude one from identifying it with bubonic plague, and he concludes that it was in fact a severe form of relapsing fever with the common accompaniment of jaundice, probably brought on by a period of famine. This renders connection of this pestilence with the outbreak of bubonic plague in the Eastern Roman Empire in Justinian's time (4) impossible, but the 'blefed' of *c.* 539 might be related to this, and to that recorded in Europe in 542—*horum exordia malorum generalis orbis terrarum mortalitas sequitur et inguinum percussione melior pars populorum* (5). In the early Irish Saints' lives, however, such as those of Declan, Rua, Mochua, and Finnian of Clonard, the 6th century plague is always described by some phrase indicating it to be the *flava pestis* and there seems no certain remembrance of any other (6). If Sir William MacArthur is correct in connecting this outbreak with a period of famine, it is possible that the story of the famine in Cornwall, in the episode of the visit of the Alexandrian ship to Britain in the 6th century, contained in the Life of John the Almsgiver (died 616), may have a foundation in fact (7). If bubonic plague did indeed reach these islands in *c.* 539 it is in the context of the trade contacts with the Mediterranean implied by such stories, and by the actual archaeological evidence (8), that its appearance can best be understood.

There is one side issue with regard to the death of Maelgwn in the Yellow Plague of *c.* 549 that is worth following. The earliest MS of the *Annales Cambriae* (Harley 3859 compiled in the late 10th century) simply records his death by *mortalitas magna*, but we can trace the elaboration of the story in successive centuries in an interesting manner. The *Vita Sancti Teiliavi* in the Book of Llandaff, compiled *c.* 1150, takes the fact of Maelgwn's death from the *Annales* but describes the *flava pestis* and its accompanying portents in detail (9), but early in the 13th century Welsh tradition in the form of the earliest MS of the Brut describes further how Maelgwn 'went into a church hard by his own castle at Deganwy and it was there he died (10)'. A later version of the *Annales*, of *c.* 1286, inserts another fragment of tradition after the entry of Maelgwn's death by *mortalitas magna—Unde dicitur Hir hun Wailgun en llis Ros. Tunc fuit llalwelen* (11). This seems to quote a proverbial saying 'Long the sleep of Maelgwn in the court of Rhos' and 'lallwelen' is presumably a corruption of 'y fad felen', the Yellow Plague. By the 15th century however this death in the church of Rhos near Deganwy is presented in

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[3] *Journ. Brit. Arch. Ass.*, 3rd s. IX (1944), 48–71.

[4] Cf. Procopius, II, XXII.

[5] Chronicle of Victor Tonnennensis, ed. Mommsen in *MGH Chron. Min.* II (1893), 201.

[6] Plummer, *Vitae Sanct. Hib.* (1910), II, 48, 242; Whitley Stokes in *Anec. Oxon.*, 1890, 229, 287 (Book of Lismore).

[7] Leontius, *Vita Scti. Joannis Eleemosynarii*; Migne, *Pat. Graec.*, XCIII, 1614 ff. Cf. V. c.H. *Cornwall*, Part 5 (1924), 19.

[8] Cf. Radford in *Trans. Devon Assoc.*, LXXIX (1947), 27.

[9] Book of Llan Dav (ed. Gwenogvryn Evans, 1893), 107; cf. Doble, *Saint Teilo* (Lampeter, 1942) and in *Journ. Theo. Studies*, XLIII (1942), 204; XLIV (1943), 59.

[10] Dingestow Court Brut (Nat. Library Wales MS. 5266), f. 288.

[11] MS. 'B' in Buffus Hardy's edition in *Mon. Hist. Brit.* and in Rolls Edition (1860), written on fly-leaves of an abridged Domesday in the *P.R.O.*

more dramatic form, describing how Maelgwn, having taken sanctuary, 'saw the Yellow Plague through a hole in the door of the church' (12). It is at this stage that the story is taken up in its well-known prophetic form in the spurious *Hanes Taliesin* (13), where the bard describes the monster with yellow eyes and teeth and hair which will come upon Maelgwn from the marshes of Rhianedd—a legend first given currency in English by Thomas Love Peacock's rendering of the 'Dymma Fustl y Beirdd' in his *Misfortunes of Elphin* published in 1829, a decade before its appearance in Lady Charlotte Guest's translation of the *Mabinogion*. STUART PIGGOTT.

ANCIENT MINING AND METALLURGY

Shortly after the end of the late war, the Royal Anthropological Institute formed a committee, with representatives of various branches of science concerned, to investigate various problems of ancient mining and metallurgy, and this Committee is at the service of archaeologists and others who wish for assistance upon such problems. It is also very willing to advise excavators concerning the technology of metal tools and other artifacts, and in certain cases to carry out complete analyses of such material.

One of the most crucial problems in study of the development of technology and applied science is the extent to which native copper was used in prehistoric times, and how far the discoveries of its properties of malleability and fusibility preceded that of the art of extracting copper from its ores. It is generally admitted that native copper was used before smelted copper. Indeed, one school holds that early metal-using cultures were dependent upon native copper for a considerable time, so that there would be two phases in intelligent metallurgy (apart from a supposedly still earlier phase in which, as in pre-Columbian North America, copper was worked cold as a superior kind of stone). We badly need data to determine how far cultures using only native copper preceded those using the smelted copper, but this is bound up with a further problem, namely the best method of distinguishing the native copper from the metal from oxidised ore.

These questions are not easy to answer and as it seemed most necessary to clear up such an unsatisfactory position, the Committee started to investigate the problem and has issued a preliminary report (see *Man*, 1948, 3 and 17). In a measure this report cleared the ground by stating the difficulties to be overcome. In order to make further progress, a large body of material must be examined, and therefore archaeologists are asked to advise the Secretary of the Committee (Miss S. Benton, F.S.A., c/o Royal Anthropological Institute, 21 Bedford Square, London, W.C.1) of material from early cultures of which they have knowledge, or which they could send for examination and report.

AMERICAN EXCAVATIONS

The University Museum of the University of Pennsylvania is currently engaged in archaeological research in Iraq, Iran, Turkey, Cyprus, British Honduras and Alaska. All of these researches have begun since 1947 with the return to normal after World War II.

In Cyprus, at the encient site of Kourion, the Museum is continuing excavations begun several years before the last war. At the moment Dr B. F. Hill, George McFadden

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[12] Brut in Cotton Cleo B. V. I am indebted to Prof. Henry Lewis for this information.

[13] Text in *Myvyrian Archaiology* (2nd edn., 1870), 29; cf. Ward, *Cat. Romances . . . in Brit Museum* I (1883), 421 for the MSS.

and DeCoursey Fales are completing the Roman Theatre and the Roman Baths on what is known as Kourion Bluff, also the Sanctuary of Apollo which has produced a remarkable collection of terra cotta figurines dating from several periods in the history of Kourion. Dr and Mrs John Young are preparing the final study of these figurines for publication by the Museum this year. Further publications completing the work at Kourion, particularly those covering the Bronze Age site at Bamboula near Kourion, should be completed within the next two or three years.

In Iraq the Museum has joined with the Oriental Institute of Chicago to continue excavations at the site of Nippur where some sixty years ago the University Museum began its work in the Near East. This is the Mecca of the Babylonians which has produced the majority of the literary tablets or what has been described as the world's earliest known literature. This season of research under the direction of Dr Thorkild Jacobsen and Dr Donald McCown has meant the completion of the Temple of Enlil dating from Ur III and discovery of additional literary tablets which give us fresh knowledge about the religion, mythology and philosophy of the Sumerians. Two of these reported by the Press describe a curious murder trial about 1900 B.C. and something of the moral and religious philosophy of the same period in a tablet concerning the goddess Nanshe. Excavations at Nippur will continue in 1951.

In Iran Dr Carleton Coon has discovered the first extensive remains of an Upper Palaeolithic culture as well as nicely stratified Mesolithic and Neolithic cultures in cave deposits known as the Belt and the Bisutun caves. The large collections representing Upper Palaeolithic, Mesolithic and Neolithic periods are now in process of study but it is possible to say that they indicate some startling conclusions. The Museum intends to continue excavations in Persia also in 1951.

The site at Gordion, some 60 miles from Ankara in Turkey, was begun by Dr Rodney Young and his associates from the University Museum in April of this year. With luck which comes to archaeologists only once in a long time they struck an exciting tomb containing a hoard of gold objects on the first day of excavation. It is a Phrygian tomb which probably dates from the 7th century B.C. and encourages the excavators to believe that Gordion is a most productive site. The excavation of additional tombs and the big city site of Gordion will continue through May, June and July of this year.

Dr Linton Satterthwaite has just cabled the Museum from British Honduras that he has discovered some new and remarkable Old Empire Maya monuments at the ancient Maya city of Caracol. He is laying plans to continue work at this site and at Cayo X where he will dig some house mounds next year. The Museum plans to continue in British Honduras with most hearty cooperation from the Department of Antiquities. To British readers it may not be surprising that other Middle American countries are not at present too cooperative in this kind of research.

In Alaska the University Museum is working in collaboration with the Danish National Museum and the University of Alaska with the joint research under the direction of Dr Froelich Rainey, Dr Helge Larsen, Denmark, and Dr Louis Giddings, University of Alaska. Last year Giddings and Larsen discovered on Seward Peninsula ancient flint industries which appear to link Upper Palaeolithic industries of the Old World with the so-called Folsom and Yuma complexes of the New World and thus give us the first real evidence of ancient connections between the two continental land masses.

The specific researches referred to above indicate the general fields in which the University Museum expects to concentrate during the next several years and reflect the intention to carry on in areas where the Museum has concentrated during the past half century.

FROELICH RAINEY.

Reviews

TABULA IMPERII ROMANI. BLATT M 32 MAINZ. *Edited by* PETER GOESSLER *Römisch-Germanische Kommission, Frankfurt-Main,* 1940.

About fourteen years ago, when the founder of ANTIQUITY had succeeded in collecting together a body of international historians, archaeologists and geographers, to discuss the furtherance of an international Map of the Roman Empire on the scale of 1 : 1,000,000 I had the good fortune to attend the Conference and listen to its deliberations. Apart from the energy and tact of the Secretary, Mr O. G. S. Crawford, what were chiefly noticeable were the determination of some countries who possessed somewhat small portions of the former soil of the Roman Empire not to be left out of it, and the elaborate and detailed scheme that resulted from the discussions. It was a bold venture, and fortune does not always favour the brave, yet in the four years that remained before the outbreak of World-War II definite advances were achieved. This country has to its credit two out of three sections—O. 30 (Aberdeen) and N. 30 (Edinburgh) ; France one, L. 31 (Lyon), and Italy four,* L. 32 (Milan), K. 32 (Florence), K. 33 (Rome), and J. 33 (Palermo), while Egypt too has produced her whole quota, H. 35 and 36, G. 36 and F. 36. In 1940 Germany added one, M 32 (the name of which had been changed from München to Mainz), and to the best of my knowledge that is the complete total so far. Twelve sheets in five years is not too poor a rate, and we can only hope that with the return of peace (if that is the correct name for the present world-situation) the production of the remaining sheets may begin again, and a reasonable rate of progress be maintained.

This section, edited by Peter Goessler, comprises not only the Map itself, but also in a separate brochure an excellent short Introduction ; after this comes a Gazetteer of geographical and political names, and an Index of sites marked on the map (both those which have place-names, and those marked by symbols), and finally co-ordinates for the symbols. The map itself includes (as the Editor says) most of the Roman provinces of *Germania Superior* and *Germania Inferior,* with not inconsiderable portions of *Belgica* and *Raetia.* Its western border runs just West of Aix (Aachen) and Metz, the eastern by Kehlheim on the Danube. In spite of a large mass of detailed information incorporated upon it, the sheet as a whole remains beautifully clear to read and study, and the editor deserves congratulation upon his success.

The map is divided almost diagonally into Roman and non-Roman territory. Far beyond the frontier and to the North-East of the Rhine stand out three Roman legionary camps, Haltern, Oberaden, and Kneblinghausen. These three camps are all that have been identified, up to the present moment, of what must have been hundreds of marching-camps thrown up by legionaries campaigning at various periods during the reign of Augustus ; is it not possible that air-photography will reveal some more, and so help to elucidate the history of the Roman invasions of trans-Rhenane Germany, and even possibly (though this may be too sanguine) reveal finally the last camp of Varus ? One season of intensive photographing, carefully planned and carried out, could reveal an immense amount of fresh material, as anyone who has glanced at the recent photographs of Dr St. Joseph—disclosing sometimes not only new and hitherto unsuspected sites, but further additions to sites already known—could testify.

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*The Editor informs me that though these sheets were printed he does not think they were ever published.

I do not possess sufficient topographical knowledge of the region which this map covers for me to put forward detailed criticisms, but several points are bound to strike anyone studying it. We can observe how rich and thorough was the Romanization of the Palatinate and Eastern Lorraine; the map indicates a quantity of ruins and remains of buildings unidentified, and many villas and temples; the stone was quarried in abundance, there are many symbols denoting mines and potteries. For the density of habitation remains the region is worth comparing with Central Southern England in Roman times. Secondly, what stands out on the map is the amazing line of the long aqueduct which provided Cologne with water, and one may remark also that the Romans spared no expense in providing aqueducts even for comparatively small towns, as e.g. Rottenburg. Thirdly, we note the extent to which the Roman names for places have still survived in a fairly recognizable form, suggesting the probable survival of much of the older population during the period of invasions. And finally, glancing again at the map as a whole, we must congratulate editor and printers alike upon the exceptional clarity of it, and upon the reliability of the Gazetteer and of the indices.

One incidental note: those who are interested in adding to the picture here presented will find it well worth while turning to the maps and plans in the excellent article by Mrs Brogan, 'Trade between the Roman Empire and the free Germans' (*J.R.S.*, XXVI, 1936, p. 195), and from these plotting out roughly upon the Mainz sheet the principal finds of Roman coins and objects in trans-Rhenane Germany; one thick concentration that seems to me noticeable is that around the waters of the middle and upper Saale upon routes from Mainz itself and from Paderborn.

In conclusion let me express a hope. Is it too much to expect that we may yet see sheets M. 30 (South England and North Brittany) and L. 30 (Western France) completed? It may be that this cannot be carried out under the international system formerly planned, but might it not be done by scholars of the two nations, upon the agreed scale and under the recognized symbols? Similarly for the Dutch and Belgian sections: we know that our Dutch colleagues have been doing notable work along the lower Rhine, and our Belgian colleagues upon the problem of the late Roman frontier—to mention two aspects only; if something equivalent to sheets M. 31 and N. 31 could be produced, what an advance that would mark. To some extent on sheet N. 31 (Amsterdam) additional symbols might have to be employed, so as to indicate roughly the main finds north of the frontier in barbarian territory, but that should not be difficult. The completion of this whole section, comprising central Western Europe, would prove a work of immense value for classical and historical students alike. Cannot UNESCO be persuaded to take an interest in the project and to further it? M. P. CHARLESWORTH.

THE EXCAVATION OF STE MARIE I. *By* KENNETH E. KIDD. *Toronto University Press*, 1949. (*London: Geoffrey Cumberlege*), 191 *pages, with plates, maps, plans and line drawings. Price* 26*s*.

Ste Marie I was for ten years the headquarters of Jesuit missions to the Huron Indians. It was at once a fortress, a retreat for priests, and a centre for the diffusion of Christianity and European customs. Building operations were begun in 1639, and the site was abandoned in 1649. In 1648–49 the Iroquois attacked the Hurons, conquered their territory and destroyed their power. Ste Marie itself was not attacked, but the Jesuits fired it on 15 May 1649 and followed the surviving Hurons to their temporary refuge on Christian Island. Here they began to build a second ' residence ', Ste Marie II, but in 1650 this too was abandoned, and Jesuit missionary activity among the Hurons came to an end.

In 1940 the Jesuits bought the site of Ste Marie I, and in 1941 Kenneth E. Kidd's excavation party was in the field. The present volume is the excavation report. It is more important and it will have a wider appeal than most excavation reports, for it will attract and inspire many scholars who have no special interest in either Jesuit missions or Canadian history. It is a landmark in archaeological and historical studies. It shows what can be done when archaeological methods are directed against a comparatively modern site, against a site for which, as for Ste Marie I, there exists an abundance of historical evidence. The short and tragic history of Ste Marie I is known in considerable detail, but there are many gaps. Only the archaeologist and the spade could provide information on the size and nature of the buildings, on the methods used in their construction, and on the general culture (i.e. ornaments, pottery, tools and general equipment) of Europeans and Indians in this area.

In Britain archaeologists seldom venture far into the historical period, though there are many sites, deserted villages, derelict chapels, medieval farm buildings and the like, that are crying out for attention. Scholars like C. W. Phillips in England and W. Douglas Simpson in Scotland are leading or driving archaeologists further into the historical period, and we may soon accept without surprise the excavation of even a 19th century site. Archaeology can make as great a contribution to the history of the Industrial Revolution as to the history of the Anglo-Saxon Settlement. Canada has set an example which could be followed with profit in Britain.

The excavation of Ste Marie I is important, of course, for its own sake. It has settled many problems, and it has provided much new information on a spectacular episode in Canadian history. One might have expected excavation to reveal more clearly the limits of the largest building on the site. One might have hoped for more evidence about the superstructures. And, in Europe at least, one might raise the traditional cry for more sections. But it would be as unwise as it would be unfair to criticize, even obliquely, a pioneer excavation carried out under conditions quite unfamiliar to us. The thoroughness of the destruction in 1649, rather than any lack of excavational skill, should perhaps be held responsible for the gaps that still remain in the story of Ste Marie I. Mr Kidd's operations seem to have been characterized at every stage by care, foresight and scientific precision.

The arrangement of the report is admirable. The historical background is briefly sketched, and there follows an account of the excavation, a long section on finds, a short summary, three interesting appendices and a useful bibliography. Extremely valuable is the author's explanation of how the initial survey and the excavation itself were planned and executed. The methods as well as the results are laid before us, and for this we cannot be too grateful.

F. T. WAINWRIGHT.

A CONCISE ECONOMIC HISTORY OF BRITAIN FROM THE EARLIEST TIMES TO A.D. 1750. *By* SIR JOHN CLAPHAM. *C.U.P.*, 1949. (XVII and 324 *pp.*) 12*s* 6*d*.

On the dust jacket of this book appears a Cambridge engraving of 1690. In the foreground the open fields are being reaped and in the background (perhaps a symbol) stands Sir John's College and his University. The engraver must have stood very near the present site of the Library where the Professor of Economic History worked.

This book will be widely read and widely used ; the dust jacket will perish, and the publishers have not reprinted it (or any maps) as endpapers. Perhaps they assume that the reader will by then have assimilated Cambridge and not require the visual aid ! For the book represents not only what the author himself taught, re-cast in book form at the end of a long teaching life, but what additions to economic history have been made by those whom he taught and those whom they taught. It puts down on paper a tradition which has been largely oral, and its unconventionalities can be seen by comparing this book with the other *Economic History* in the field, Lipson's, written in 1915 and revised in 1937 and 1946.

The striking characteristic of this book is its ability to move in both time and space : places as well as people are actors, and the author is a true traveller. The essential variations in regional life, in the non-typical manor, are well brought out in pp. 86–93 ; probably more clearly to the reader than the account on pp. 110–24 of when and how the 'typical' manor disintegrated. Has the question recently posed by Dr R. H. Hilton (1) really been answered : what is there in our description of the typical manor to tell us why it should not have gone on indefinitely without change ?

In the origin of the Saxon open fields Sir John plumps for the Scottish analogy : the open fields come from the earlier in-field. This reviewer has helped to put some in-field back on to the medieval English map in certain geographical conditions, but he is reluctant to call in this intermediate stage to explain what, to him, Dr C. S. Orwin has satisfactorily explained : given men on the defensive ; given rare and expensive capital equipment like the plough ; given a social structure worked out in the forests of north western Europe before the invasions ; given the great pull on man and woman power in the first clearing . . . given these, the furlongs and strips and scattered holdings of the nucleated open field village arise in the very acts of colonising the waste.

With characteristic candour and caution the author puts down his view of the prehistoric and Roman fields : this reviewer can do nothing but agree.

Mr John Saltmarsh has prepared the MS for the press with an admirably full contents summary, page headings and cross references. There is no book-list, and some lack of uniformity in whose work shall have footnote allusion and whose not. Mr T. A. M. Bishop whose work is clearly the basis of some sentences gets no footnote, but a short phrase from Mr Hewitt's *Medieval Cheshire* does. Perhaps the reason is that Cambridge might be expected to have read Mr Bishop but not to be as appreciative of Cheshire work as Sir John (and this reviewer). This reviewer also derives some satisfaction from having helped to dig his way literally into a footnote on page 80. It is something of an achievement to have caught Homer nodding but Mr Saltmarsh confesses that Sir John overlooked the quantity of abandoned medieval village sites whose rarity he more than once affirmed (2). As long ago as 1936 Clapham's colleague H. C. Darby had written of Canon Foster's pioneer listing of the many Lincs. sites (3), and W. G. Hoskins' Leicestershire article (4) may have seen daylight before this book was completed. It is a curious oversight in a man whose whole book demonstrates that the good historian keeps his eyes wide open in his space travels as much as in his time travels. Readers will find this

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[1] *Studies in Leics. Agrarian History*, ed. W. G. Hoskins, 1949, p. 19.

[2] Indeed one even provides a windmill, earlier than his earliest example. It was at Weedley [E.R.] in 1185 : see B. A. Lees, *Records of the Templars*, p. 131 [London, 1935.]

[3] H. C. Darby, ed., *Historical Geography*, 1936, p. 208.

[4] *Trans. Leics. Arch. Soc.*, XXII, 242.

book a good travelling companion, and the Press have published it at an almost pre-war price-level. It is to be regretted that those who borrow from libraries will probably never see the dust jacket, since most libraries expose them at birth. Perhaps the next edition will see some illustrations in the text: we still badly need an accessible book with part of an open field map actually reproduced and not sketched or transcribed.

M. W. BERESFORD.

EARLY KHARTOUM. An account of the excavation of an early occupation site carried out by the Sudan Government Antiquities Service in 1944–5. *By* A. J. ARKELL. *Oxford University Press, London,* 1949. XVI+145 *pages,* 12 *text figures,* 113 *plates.* 12 in. × 9½ in. *Price* 5 *guineas.*

This handsome if rather expensive volume is noteworthy as being the report of the first archaeological excavation financed, from inception to publication, by the Sudan Government itself. It is primarily an account of the excavation—or partial excavation—of a mound on the southern outskirts of Khartoum, but, says the author in his preface, it 'is not the mere record of an excavation . . . but a work which will have to be taken nto consideration by prehistorians in north Africa and elsewhere'. This is a justifiable claim.

Those interested in the archaeology of the Sudan have for long had legitimate cause for complaint that some of the excavations carried out in that country in the past have been far from adequately published. Mr Arkell has seen to it that no reproach on the score of inadequacy should be levelled at his own publication, for in this record of a comparatively modest excavation there are no fewer than 113 large plates, some in line but most of them in half-tone. Even potsherds are illustrated by full size photographs, and excellent photographs they are.

A preliminary account of this excavation has already been published in this journal.(1) It is sufficient here to say that the low mound which was the site of the excavation was one of the two main cemeteries of Khartoum during the siege of the city in 1884–5, and it was covered with Mohammedan graves. But there was also surface evidence which suggested a very much earlier history, and excavation yielded barbed bone spear-heads, flaked implements of quartz and rhyolite, hammer stones, grinding and rubbing stones, and potsherds of a type not previously published. There were also animal bones, agglomerations of fish bones, and the fossilized remains of several early human burials, as well as some of the Meroitic and other periods. Most of the animal bones were those of animals still common in the southern Sudan, but amongst them, surprisingly, was part of the skull of an extinct and previously unknown reed rat, *Thryonomys arkelli,* related to forms found in deposits in the northern and western Sahara.

From all this Mr Arkell has built up a picture of a community of primitive negroid hunting and food-gathering people who apparently had no domesticated animals, but who had well-made red-brown pottery decorated with wavy lines combed into the soft clay. He shows that the implements used to produce this decoration were the protective spines of a type of cat-fish which still abounds in the Blue Nile. From the evidence available he concludes not only that the people of the early settlement exhibited a mesolithic culture but that the settlement itself was of mesolithic date. He finds at least three early cultures represented on his site and he traces out their connexions with comparable cultures in Egypt and other parts of north Africa. He illustrates these by a map showing the probable lines of communication between Early Khartoum and sites in the southern Sahara and the Tunis region.

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(1) ANTIQUITY, No. 84. December, 1947.

The characteristic culture of Early Khartoum was the Wavy Line culture, that of the people who made the pottery decorated with wavy lines, and this is regarded as being ancestral to the Badarian in Egypt. The bone spears from Early Khartoum are also considered to be earlier in date than the earliest known in Egypt. Somewhat later than the Wavy Line culture came the Gouge culture, represented on the site by only a few sherds; still later, and possibly of proto-dynastic date, came the Omdurman Bridge culture, this last deriving its name from the pottery found in two graves discovered near the bridge connecting Omdurman with Khartoum. The Wavy Line culture was not confined to the Nile Valley but was spread more or less across the Sudan in the latitude of Khartoum. Mr Arkell gives a list and a distribution map of sites in the Sudan on which Wavy Line pottery has been found and he concludes: 'It may therefore one day be established that pottery of this type, perhaps invented in Asia, was spread by negroid people right across Africa through what is now the (northern) Sudan and the southern Sahara, before it entered the lower Nile Valley to provide one of the features of the early civilization of Egypt'.

There is a fine imaginative sweep in all this. Here Mr Arkell displays his gift for relating the particular to the general and, whether or not his highly original and interesting conclusions are ultimately confirmed, he has made a notable contribution to north African pre-history.

Against this wide background the actual history of the Khartoum mound may appear unimportant. Yet one of the objects of an archaeological excavation is to reconstruct as fully as possible the history of the chosen site, and there are certain aspects of the history of Early Khartoum which might have been more fully explained.

We are told that the early settlement was small in area and was situated on the top of a sandbank on the edge of a Blue Nile which then rose in flood about four metres higher than it does to-day. Since the time of the early settlement about two metres have been eroded from the top of the sandbank resulting in a marked flattening of the topography and the burial of what was the Blue Nile flood plain at the time of the settlement. (A section illustrating this was published in ANTIQUITY (2) and so does not appear in the book). During this period in the history of the mound precipitation of calcium carbonate (kankar) had formed, due to the fluctuations of the water-table, and its presence had resulted in the fossilization of the human bones and the calcretion of the objects belonging to the early settlement. The deposition of kankar had ceased before Meroitic times so that the bones in the graves of this period are not fossilized. Also, a good deal of evidence is provided to show that, when the site was first occupied, the rainfall in the latitude of Khartoum was much heavier than it is to-day.

Since no stratification could be detected on the site the human burials form the only chronological sequence that can be observed. It is admittedly imperfect because graves vary in depth and it does not necessarily follow that bodies found at the same level have been buried from the same surface. With these reservations it is to be expected that, in a mound which has been subjected to erosion, the burials of different periods ought to appear at progressively lower levels. In the report before us the burials are dealt with in separate groups, and the squares in which they were found are indicated in separate text figures. But this treatment, however convenient on archaeological grounds, does not give a complete picture. All the graves were in the same mound and were equally part

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[2] *loc cit.*

of its history, and if their relationship to each other is to be properly understood as many of them as possible should have been shown on the same vertical projection. If this is done it will be seen that the Meroitic burials lie in two groups at different levels and it is not surprising to find that Mr Arkell, for purely archaeological reasons, considers the group at the higher level to be the earlier in date. The two are at such a distance apart as to suggest that rapid erosion of the mound occurred during the interval of time by which they are separated. It will be seen, too, that the Meroitic burials on top of the mound—the area which has suffered most erosion—are at levels which differ little from those of some of the burials of the early settlement in near-by squares. This is a curious circumstance which can be interpreted in various ways. In any case it would seem that the erosion of the mound was neither continuous nor constant in rate. These facts, possibly, concern only the history of the mound, but they are relevant to the wider issues in so far as the date of the early settlement is concerned. As to this, Mr Arkell would have strengthened his claim to a mesolithic date if he had considered these points and reconciled them with the deductions he has drawn from a study of his finds.

There are some interesting parallels between the objects from Early Khartoum and those from the much more extensive excavations of the late Sir Henry Wellcome at Jebel Moya (3), about 150 miles south of Khartoum, which had not been published when Mr Arkell wrote. Dr Derry, in his report on the human remains from Khartoum, says, of the only skull from that site which could be reconstructed, that its description exactly fitted that of the skulls from Jebel Moya which he examined in 1911–12. The only difference was that the Khartoum skull lacked the upper incisors, whereas at Jebel Moya the lower incisors were sometimes removed Dr Derry also observed that the bones from Khartoum were so impregnated with calcium carbonate as to ring like pottery when struck. So also were many of the bones from the (nearly 3000) graves at Jebel Moya, although they were not completely fossilized. Yet the skulls from this site which Dr Derry examined came from a cemetery in which some of the graves contained Napatan (*ca.* 750–550 B.C.) beads and copper hair ornaments, sometimes *in situ* on the body. At Jebel Moya, too, there were flaked stone implements, chiefly of quartz, of much the same kind as those from the Khartoum site, also hundreds of grinding stones similar to those from Khartoum, and quantities of stone rings, mostly broken. All these types of stone objects occurred in the same strata as Napatan amulets and datable scarabs, and Jebel Moya was a well stratified site. On the lower levels there were potsherds not dissimilar, to judge only from photographs, to some of those from Khartoum, and the fragments from Jebel Moya are not likely to be of a date much earlier than 1000 B.C. The sherds from Taferjit and Tamaya Mellet in the French Sudan which Mr Arkell illustrates on Plate 102 appear—again judging only from photographs—to have closer affinities with the early pottery from Jebel Moya than with that from Early Khartoum.

The only inferences which can safely be drawn from these facts are that certain types of flaked implements had a range in time of several thousand years, that some kinds of pottery in the Sudan had a long history and a wide distribution, and that human bones can acquire a considerable degree of fossilization in less than 3000 years. They lead to nowhere in particular, unless they point to the need for some method, which in most cases does not exist, of cross-checking the dates of ancient sites by reference to criteria which are not purely archaeological.

In spite of all this, it may well be that Mr Arkell's reconstruction of the movements of early peoples is close to what actually occurred. His book is a considerable achievement,

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[3] *The Wellcome Excavations in the Sudan*, parts I and II. *Jebel Moya*, by F. Addison.

and his detailed publication of his finds will provide archaeologists and prehistorians with much new material for study and a good deal to think about. It is to be hoped that the Sudan Government, having once committed itself to the publication of full archaeological reports, will not draw back and that Mr Arkell's next volume will not be long in appearing. F. ADDISON.

CAMBRIDGE EXCAVATIONS IN MINORCA. *By* MARGARET MURRAY, D.LITT., F.S.A. (Scot.), F.R.A.I. *London, B. Quaritch.* TRAPUCÓ, Part I, 1932. SA TORRETA, 1934. TRAPUCÓ, Part II, 1938. *12s 6d each volume.* (The last named has only just been published).

For the purposes of this review the reports are numbered I, II and III. They cover three season's work on two sites near Mahon in Minorca, one, Trapucó, a mile south of the town, the other 5½ miles to the north of it.

Two enclosures each containing a 'taula' were cleared, one at either site; also the remains of what may have been a 'naveta' at Trapucó; and irregular chambers at both sites.

It was to be hoped that these excavations might have solved the problems of the date and purpose of the constructions known as 'taulas', which are found in that island, and probably existed in Majorca. They occur in association with the strong circular towers known as 'talayots', which closely resemble similar buildings in Sardinia termed 'nuraghi', which were the homes of local chieftains.

The taula consists of a rectangular monolith carrying a flat table stone, the pair giving a T section. The two stones forming this construction are often carefully dressed and stand excentrically within a D shaped area contained by a ring of upright slabs embodied in a dry stone wall; the slabs are set athwart the wall with the long axis in each case pointing towards the taula. The taula at Trapucó was found to rise to a height of 16 feet and the enclosing slabs apparently to some 10 feet.

The obvious explanation of this arrangement is that on the central cap stone rested a series of cross beams which were carried by intervening pillars to the slabs in the outer wall and that these beams supported the roof. Such a construction is frequently found in Minorca and Majorca on a smaller scale—and is illustrated in a cruder form in plates VII and VIII of vol. II.

Miss Murray rejects this explanation however in favour of a more romantic theory that the upper stone of the taula was an object of veneration 'the emblem, the outward and visible sign of the Deity, raised on high to be viewed by all the people'. (She is uncertain of the sex of this deity). She records the remains of corbelling in two of the corners of the enclosure in which stands the Trapucó taula (I, pl. IX, 3). The 'built pilaster' in that illustration, consisting of 5 superimposed rough stones, should, by analogy with other sites, have been plastered over to simulate a monolith, and if similar constructions formed the intermediate roof pillars they might well have vanished leaving no trace for any but careful and expert excavators. Free-standing uprights still carrying rough cap stones and supporting a roof are illustrated in Vol. II, plates VII and VIII.

Whatever the ceremonial use of the taula buildings may have been—and the careful finish of the taula itself points to some especial importance—they seem to have formed a 'hall' which was surrounded by a series of lesser chambers, most or all of which were subterranean or without windows. It should be remembered that the earliest accounts of the people of the islands describe them as wearing little clothing, if any, and inhabiting natural or artificial caves.

Unfortunately we are given no general site plans of the groups of buildings to indicate the relative positions of talayots, taulas and the many subsidiary chambers, nor are there proper or comprehensible plans of the areas excavated; indeed for the site reported upon in Vol. II there is only a sketch plan without scale, as 'the measurements and the plan drawn on the spot have disappeared'.

There are no sections of the ground excavated, although the varying types of the pottery alone indicate long residence and re-use of the sites.

There are however many good photographs and drawings of the very varied assemblage of pottery; but no scales appear on the drawings in Vol. III and for the photographs only occasionally a portion of a rule is included with the fragments of pottery. When they are legible these rules represent either 10ths or 12ths of unspecified units of length.

For the dating of the different types of monument little evidence is offered; but Miss Murray records her opinion, based on comparison of the structural details of the temples of Malta with the building methods of the Minorcan enclosing walls, that the latter 'belong to the first half of the Bronze Age'; as in her view, they cannot be contemporary with the taula, 'it would appear that the taula must have been erected originally in the open and enclosed at a later period'; also that 'the date of the taula is of the beginning of the Bronze Age' (III, p. 36) but 'the nature of the working would show that the taula of Trapucó belongs to the Neolithic period' (I, p. 11).

Miss Murray produces a new theory to account for the form of the tombs of megalithic masonry which were certainly designed to reproduce the rock cut caves with all their elaborate detail. They are locally known as 'navetas' from their superficial resemblance to an up-turned boat; Miss Murray's verdict is 'It is probably a representation, on a large scale and in stone, of an original boat-burial'. (II, p. 22).

The volumes are in fact of no scientific value as records of these Balearic monuments, and their usefulness for students of the pottery, which might have been very great, is reduced to the minimum by the failure to record stratification.

An exception should be made however in favour of the very careful record of human remains by Dr John Cameron in Vols. II and III. And, when scientific excavation of other sites has established the dates of the different classes and forms, these illustrations of pottery and the original material, which is presumably preserved at Cambridge, should be of considerable value.

The cost of the three expeditions was borne by the Cambridge Museum of Ethnology, thanks to the very generous contributions of its Director. W.J.H.

EARLY SCOTLAND. *By* H. M. CHADWICK. *pp.* XXIX+171. *Cambridge University Press*, 1949. 15*s*.

Cambridge paid to Chadwick the honour it accords to its greatest scholars when it made of him a myth. Elusive, as was supposed, to the point of being unapproachable, the student of many languages and of remote branches of learning was traditionally hidden behind his published works. We had learned from the *Heroic Age*, and from the world-wide survey in the *Growth of Literature*, how earlier peoples are to be known through their orally transmitted literature; how tradition and myth may tell us more of a race than does its veridical history; how the entertainment in the hall and the stories related round the fire are a more living part of our heritage from earlier ages than is a knowledge of their politics and their economics. This widening of our approach to the past was Chadwick's share in the revolution in thought wrought half a century ago by the *Golden Bough*.

In this posthumous work, and in Mrs Chadwick's introduction to it, we are admitted to see a little more of the elusive scholar. It was not for nothing that Chadwick was named Hector Munro. In intervals of his already known studies he was at work on Celtic Scotland and, as the illustrations show, was travelling to obtain that intimate knowledge of the geography of the land of his ancestors without which the peoples and their history cannot be understood. What we have as a result is a series of studies of Pictish and to a lesser extent Dalriadic and British history, which will be prolegomena to all future investigations in these ill-cultivated fields. The wide range of questions condensed into the chapters of this short book is summarized and brought together in Mrs Chadwick's able introduction, which serves at the same time to remind us how far ahead final judgment on so many of these issues still lies. It was Chadwick's ambition to found a school of study of the Late Prehistoric and Dark Ages, and it may be hoped that the new points of departure provided by this book may serve as a challenge to future research by students in all the branches of learning which this catholic scholar was able here to bring into contribution.

For the book is notable for the width of its approach. In the interpretation of the annalistic sources, which was successfully undertaken so long ago by W. F. Skene. Chadwick has fewer, though still notable, contributions to make ; and the opportunities for further advance by purely historical methods are plainly limited. His chief contribution is the reinterpretation of the relations of the kingdoms of the Dalriadic Scots and of the Picts which, in the 9th century, brough a Scot to the throne of a combined kingdom. The study of the genealogies, and particularly of those of the South Scotland families which are preserved in Welsh sources, yields a series of inferences of the greatest interest regarding the re-establishment in A.D. 369 of the northern frontier by the creation of Romano-British kingdoms which long survived the withdrawal of the legions. In the field of Celtic linguistics a fresh synthesis is achieved and related to the movements of population, from the Continent to Britain and from Britain to Ireland, from the Late Bronze Age onwards, which are beginning to achieve definition as a result of the work of archaeologists. A study of the distribution of place-name elements breaks new ground, despite the pioneer achievement of the late Prof. Watson, and yields results regarding the distribution of Gaelic and Welsh-Pictish speech at different periods—results which are associated with both population movements and changes in political domination. Finally, and notably, archaeology is made to yield its full contribution to the problems of Scottish history throughout the later first millennium B.C. and the first millennium A.D.

From this too brief survey the scope of the book will be realized. That so much can be contributed in these closely woven chapters is a tribute to the width of the author's scholarship and to the alertness of his sympathetic understanding. But Chadwick would have wished the book to be regarded, not as an end, but as a new beginning, and to none of us is it so much a challenge as to the archaeologist. Faced with the obligation to cooperate with Chadwick as historian, as philologist and as place-name student, we realize how short a distance we have gone towards building up a reliable picture of the cultures, the tribal groupings and the population movements of early Britain. L.S.

BEFORE PHILOSOPHY, *by* H. *and* MRS. FRANKFORT, J. A. WILSON *and* T. JACOBSEN. *pp.* 275 *Pelican A* 198, *London*, 1949. 1*s* 6*d*.

Penguin Books deserve congratulations and gratitude for producing this English edition of a work by the same authors published in Chicago under the title ' The Intellectual Adventure of Early Man ' ; and I do not think the omission of a long section on the Hebrews is to be regretted. The American title was perhaps more accurate ; for what the authors here present is really the oldest extant philosophy—the world view of the first literate peoples, Egyptians, Sumerians and Babylonians, and their conceptions of Nature, Man, Society and the End of Life. Being a preliterate archaeologist, I am incompetent to assess the correctness of the presentation, since I cannot check how far the quotations with which they copiously document their account are accurately translated and representative. But I can say that what they present does fit remarkably well in the general pattern of mankind's intellectual development as it is seen by modern anthropology.

Frankfort evidently agrees with Durkeim : ' Logic presents different characters at different periods of history; it develops, like societies themselves; its laws, far from being graven from all eternity on the mental constitutions of men, depend upon factors that are historical and consequently social '. It is the second stage of development that is here described. Let us consider the first.

Our prehistoric ancestors, to judge by preliterate peoples of today, conceived of nature as alive and indeed coterminous with society. If they constructed for themselves a conceptual working model of reality, it would be built on the analogy of society, not indeed because society is the most familiar part of man's environment, but because it is the most predictable and so, within the limits of the customary behaviour pattern, the most amenable to modification. But savages and barbarians have no opportunity for *thinking out* a conceptual model of reality, as against *acting on* its more obtrusive bits and pieces. There is no class exempt, as a class, from the urgent task of wringing a livelihood out of reluctant nature ; even a Maori chief must ' be industrious in collecting food '. There is therefore no divorce of theory from practice. Thinking is expressed in action whether that be the effective activities of craftsmanship or the futilities of magic rituals and spells (of course even these have a functional justification, but not what their practitioners suppose).

The urban revolution evoked for the first time a class of literati ' exempt from all manual tasks ' (Wilson quotes the passage but without indicating its significance). Supported from a social surplus, produced by the peasant masses and concentrated in the granaries of the temple or the pharaoh, these had leisure for speculative thought ; it is the fruits of their leisure that are collected here. Naturally they did not start thinking *in vacuo*. They had inherited from prehistory those ' priceless instruments of human thought ', categories, and objectified thoughts, including the rites to which they owed their privileged position.

So the pattern for their model of reality was still society. ' Any phenomenon ', writes Jacobsen, ' which the Mesopotamian met in the world around him was alive, had its own personality and will. To understand nature, the many and varied phenomena around man, was thus to understand the personalities in these phenomena, to know their characters, the direction of their wills and also the range of their powers . . . Intuitively the Mesopotamian applied to nature the experience he had of his own human society, interpreting it in social terms '. But society had changed since prehistoric times. Now there are rulers. They need not soil their hand by pushing and pulling things. Instead they issue commands—spells that are really effective—and their servants push and pull at their behest. So nature, still animate, becomes filled with personal gods who can likewise issue commands. Their behaviour, like that of earthly rulers, can be modified by bribes and entreaties, though it still needs re-inforcing by magic.

' The emancipation of thought from myth ', representing a third stage in intellectual development and sketched very briefly by Frankfort in the last 14 pages, took place in a different kind of society again. The Ionian philosophers, in contrast to the literati of the Near East, were not drawn from a class set apart ' to concern themselves with spiritual matters ', but engaged in public life and practical activity. Even in the 6th century Greek citizens stood much nearer to tribal barbarism than did a citizen of Memphis in the 26th or of Ur in the 20th ; for the Minoan civilization had been overwhelmed by barbarism, its ruling class swamped by Achaeans and Dorians and its religion largely replaced by older or more primitive popular cults. If nature were still alive and even social, it was not effectively populated by equally personal gods (such had retired discreetly to Olympus as Cornford remarked). In so far as the order of nature was modelled on the order of society, the latter was neither that of barbarism, where every man's place is determined by custom, nor of a theocracy where it is fixed by the ruler's will, but of the new commercial polis, where wealth, reckoned numerically in coins, determined status. On the other hand, thanks to the improved tools of the Iron Age, the crafts were able to offer controlled processes as bases for the conceptual model, and the Ionians, as Farrington has insisted in another Pelican, seized this opportunity.

The preceding paragraphs do not pretend to be a summary of the book's contents. Within the two thousand years it covers, ' Before Philosophy ' has a much wider range. It is ' a study of the primitive myths, beliefs and speculations of Egypt and Mesopotamia '. That does not mean a rehash of Erman and Langdon, seasoned with newly discovered or freshly interpreted documents. The whole presentation is fresh. The vivid depiction of the Egyptian and Mesopotamian environments and their influence on the two world views is particularly enlightening. Jacobsen's account of the Sumerian temple-state as a primitive democracy ruled by a council of elders and an assembly of all freemen which might appoint a ' king ', is quite novel. He has of course stated the arguments in detail in learned journals. The most plausible are based on the pictures of the cosmic State of the gods presented in myths which, if they really reflect the organization of the earthly state, must go back far beyond Urukagina to the ' protoliterate period ' (*scil.* Uruk) or even al'Ubaid. V. G. CHILDE.

SEMITIC WRITING : From Pictograph to Alphabet. The Schweich Lectures of the British Academy, 1944. *By* G. R. DRIVER, M.A., F.B.A. *Oxford University Press, pp.* XVI, 222, 57 *plates.* 25*s net.*

The *fons scientiae* overflows the narrow vessel originally destined to contain it, and very few of the scholars who have delivered the long series of Schweich Lectures since their inception in 1908 have contrived to keep their material within the bounds of three short lectures. Professor Driver's generous overflow has only been exceeded by the late Professor S. A. Cook who has beaten him by a short head of 30 pages, but this is offset by a win for Professor Driver in the matter of illustrations, with 57 plates and 97 text figures as against Professor Cook's score of 39 plates and no text figures : so with regard to statistics the honours may be held to be equally divided.

But, jesting apart, we have here not only a generous profusion of material of the greatest interest, but a most skilful and illuminating treatment of the many problems with which this fascinating subject abounds. Considering the immense advances which have been made in our knowledge of the history of early systems of writing during the last half-century, it was high time that the subject should find a place among these lectures ; and it was fitting that the son of the eminent scholar who inaugurated the series should be the first to deal with it.

Professor David Diringer's learned book on *The Alphabet*, which appeared shortly before the present volume, has already been reviewed in this journal. While both books deal with the general problem of the origin of the alphabet, Professor Driver has limited the scope of his treatment to the Semitic field, and hence has been able to deal more intensively with matters immediately relevant to the development of Semitic forms of writing. For example, his first lecture, dealing with the cuneiform scripts, occupies 77 pages, and brings together all the most recent knowledge and all the latest theories concerning the origin of that form of writing, its development, its materials and technique, with full documentation. The only other treatment at all comparable is in the late Professor Chiera's book *They Wrote on Clay* (Camb. Press, 1939), where the subject is dealt with in a much more popular fashion, and far less exhaustively.

Professor Driver is of the opinion, no doubt justified, that economic necessity was the mother of the invention of cuneiform, since the earliest collections of tablets come from temple archives, such as those at Erech, and appear to be wholly concerned with temple accounts and revenues. He also suggests that the same reason led to the invention of the hieroglyphic script among the ancient Egyptians, possibly under Sumerian influence. Not all Egyptologists will be prepared to agree with this view, and if the palette of Narmer is the earliest example of pictographic writing in Egypt it may furnish some ground for questioning this assumption. On the other hand, the well-known pictographic tablet from Kish, reproduced in Plate 1, fig. 2, of Professor Driver's book, shows several signs closely resembling certain Egyptian hieroglyphs; in the case of some of these, such as the human head, hand, foot, and *membrum virile*, the resemblance need not be explained as due to the influence of either script upon the other; but a problem is raised by the sign representing a sledge with a hut or some other object on it. It is known that the sledge remained in use in Egypt throughout the Old and Middle Kingdom periods, but evidence is wanting to prove its use in Babylonia before the introduction of the wheeled cart, at a date prior to the 1st dynasty of Ur; hence the appearance of this sign on the Kish tablet might be taken to suggest Egyptian influence. Professor Sidney Smith's cautious judgment on the point is worth quoting; speaking of the two scripts, i.e., the cuneiform and the hieroglyphic, he says, 'There is no ground for connecting the scripts; it would be equally rash to deny the possibility of a connection'. (*Early History of Assyria*, p. 12).

The second lecture deals with the subject of alphabetic writing, and is prefaced by a full account of early writing materials and implements. Professor Driver has an interesting interpretation of the somewhat obscure passage in Isaiah XXVIII, 9–10; the English Revised Version renders v. 10, 'For it is precept upon precept, precept upon precept; line upon line, line upon line; here a little, there a little'; but Professor Driver proposes to translate it, 'For it is ṣ–ṣ, ṣ–ṣ, q–q, q–q, a lad here (and) a lad there', and takes the verse as a description of a schoolmaster teaching scholars the alphabet by calling out consecutive letters and making the boys repeat them after him. He suggests that the words rendered 'precept' and 'line' are really the sounds of the consecutive letter *ṣadde* and *qoph*, as we might speak of p's and q's. The proposed interpretation is most interesting, and removes what has always been recognized as a difficulty, the forced meaning given to the words *ṣav* and *qav*; but some difficulty remains in the reference to a foreign tongue, i.e., the language of the Assyrians, in v. 11.

The author does not regard the cuneiform alphabet of Ras Shamra as the earliest attempt at a Semitic alphabet, but as 'an experimental attempt to adapt the cuneiform to the alphabetic system in the light of the Phoenician alphabet' (p. 104). He returns in the third lecture to the question of the relation between the signs of the Ras Shamra, or Ugaritic, alphabet, and the Accadian syllabary on the one side, and the North and South Semitic alphabets on the other. He rejects, and rightly, the view which would derive the Ugaritic signs wholly from either source, and states his own position as follows:

' The inventor of this alphabet was in any case no mere copyist but rather an experimenter who was not afraid of novelties which might not and in fact did not survive his system. He was acquainted with the use of writing-clay and the cuneiform script and chose it perhaps because it was more suitable for long records than stone and less perishable than papyrus ; but, being aware of the difficulties inherent in a syllabary, he preferred to devise an alphabet on the Egyptian or Phoenician model, of which he must have been aware, since recent excavation has shown that the Phoenician alphabet antedates the Ugaritic texts and Egyptian influence has been traced at Ugarit. Excavation, too, has shown that the period *c.* 1750–1000 B.C. was one in which experiments in writing were being made, and the obvious conclusion is that the Ugaritic method was one of these experiments. The inventor would be likely to borrow what seemed to him suitable or advantageous in the experiments made by neighbouring workers and add or adapt it to his own system ; for almost every invention is based on previous discoveries. His system, however, was invented too late to oust the Phoenician method which already held the field ; and indeed it scarcely lasted a generation, since clay was not so convenient and handy a medium of writing as papyrus, which therefore won the day. Date, medium and method, combined to ensure its defeat ' (p. 151 f). A point of some importance arises from the statement that the Ugaritic script scarcely lasted a generation ; the important collection of religious texts which have occupied the attention of scholars during the last twenty years contains the ancient myths and rituals of the Semitic population of this part of the Near East. If these traditions were only written down in the Ugaritic alphabetic script during the closing years of that civilization, we have to ask in what language and in what script did they exist before they received the form in which we have them ? From the archaeological evidence it would appear that the civilization of Ugarit was destroyed by the inroads of the Sea-peoples about the end of the 12th century B.C., after which date the alphabetic cuneiform of Ugarit ceased to be used. Among the tablets from Ugarit are some, probably of 14th century date, which present the curious phenomenon of Accadian written in alphabetic cuneiform. and one, published by Thureau-Dangin, R.A. XXXVII, 3, which is bilingual, Accadian and Ugaritic. Hence it is clear that considerably more than a generation before the disappearance of the Ugaritic civilization, some interchange was going on between the Accadian and Ugaritic languages and scripts. The Tell el-Amarna letters, which are contemporary with much of the literary material from Ugarit, show Canaanite glosses written syllabically in cuneiform, and this fact may furnish a possible answer to the question of the form in which the Ugaritic traditions and myths existed before the invention of that remarkable cuneiform alphabetic script which seems peculiar to Ugarit.

Professor Driver's book is of such an encyclopaedic character that it is impossible to discuss the many points of interest which it evokes, but mention must be made of the very interesting account and sound criticism of the various theories relating to the names and order of the letters of the Semitic alphabets. We shall close our review of this learned, valuable, and most interesting book by quoting Professor Driver's admirable summary of the conclusions to which his study of the problems of the origins of the alphabet has brought him : ' The conclusion of the matter then is this. The Sumerians invented writing on clay by means of pictographic signs and devised a method of using these to render syllables ; they also accidentally isolated four of the five vowels. The Babylonians developed the use of these signs for syllables and employed this syllabic script in continuous texts of every kind, interspersed with ideographs ; the Persians invented the simplest form of syllabic script based on the cuneiform system. The Egyptians had early devised their own system of hieroglyphs which they carried forward through the hieratic and demotic stages of cursive writing; they also adapted their signs for occasional use as syllables and even as consonants but never used them so in continuous texts except for a brief experimental period. It was the merit of the western Semites that they saw the importance of this discovery and, discarding the whole cumbrous machinery of ideographic and syllabic scripts and providing that each sound was represented by only one sign, made a simple alphabet the vehicle of written thought. Who first took this step is and may always remain unknown; all that can be said is that he or they were sprung in all probability from one or other of the Semitic peoples who came into contact with the Egyptians *c.* 2500–1500 B.C. and that it was taken in or near Egypt, and that the invention was developed in Palestine and perfected on the Phoenician coast. At this early stage three types of alphabetic script were evolved, a mixed pictographic-linear, a cuneiform, and a true linear script; the two former soon died out while the latter survived to be carried by the Phoenicians overseas to Greece, whence it passed to all the nations of the western hemisphere—one, and only one, of the gifts of the Semites to mankind.' S. H. HOOKE.

ART IN MEDIEVAL FRANCE, 987–1498. *By* JOAN EVANS. *pp.* XXVIII+300, *with* 280 *plates. Oxford: Geoffrey Cumberledge, Oxford University Press,* 1948. *Price* 63*s.*

Dante as he met Virgil at the beginning of his pilgrimage expressed to his master the following hope:

Vagliami 'l lungo studio e'l grand'amore
Che m'hanno fatto cercar lo tuo volume.

Dr Joan Evans, we feel, must have approached medieval France in much the same spirit. Her knowledge of the subject appears on every page, but knowledge alone would not have produced this fascinating book. It is inspired with a deep love for her subject, the art of France in the Middle Ages. Her approach can be best explained by quoting from her preface. 'My whole endeavour . . . is to show that French medieval art took the forms it did because of the needs of the men who commissioned it . . . We falsify history if we regard any kind of art as a series of specimens ranged in museums, impersonal and without reasons or background . . . The medieval art of France has to be studied in castles where no one lives, monasteries where none takes his vows, and, often, in churches where no one prays. Only by an effort of the imagination can we endow them with their true significance, but the effort will make the stones live'.

The production of the book is excellent and the 280 plates provide a well chosen view of French medieval art. The text covers the whole of the field and is supplemented with many footnotes and references, which will enable the reader in search of fuller information to follow the vistas opened up by her treatment of the subject. This is so wide that there will inevitably be points of detail on which the specialist will differ. We have noted a few, but these do not affect the value of the work. In the wider field it would be difficult for anyone less conversant with the whole range of material adequately to challenge a thesis, which carries conviction.

The major part of the book is concerned with ecclesiastical art, starting with the great Benedictine revival of the 10th and 11th centuries. The evolution of the churches is discussed with particular reference to the great pilgrimage churches and their influence. The ornament of these buildings and the sources from which it was derived are fully considered. 'Usually', concludes the author, 'the painter's and sculptor's model was provided by the illuminated manuscripts in the abbey library'. This is only one instance of the way in which one art is shewn to be dependent on another. The purist might cavil at the inclusion of Cluniac churches under the heading Benedictine, but there is sufficient justification on artistic grounds; less happy is the extension to cover a church of canons like St. Martin of Tours. Further chapters deal with other religious bodies, the Cistercians, the Bishops and Chapters, the Augustinian Canons, the Mendicant Orders and the Carthusians, shewing how the special requirements of each are reflected in their buildings and their ornaments. The influences apparent do not spring only from the ecclesiastics; in many cases the taste of the lay donors was responsible. This factor tended to increase towards the end of the period, which provides a good example in the Carthusian church of Champmol, where the Dukes of Burgundy were buried. Here much of the sculpture 'can fairly be considered courtly rather than monastic'.

The chapter dealing with the king and court also throws much light on the development of the secular art of France during the Middle Ages. We see the bare furnishings of the stark military castle giving way to a gradually increasing luxury, which eventually culminated in the brilliant flower of the courtly art of the early Valois. Watching this evolution we begin to realize how different the surviving buildings must have looked before the colour faded from the stonework, before they were stripped of paintings and hangings, before the rich plate and silks were removed and destroyed by the envy and cupidity of a later age. A good example of the method which Dr Evans employs to bring home her argument is the juxtaposition of a view of the great hall of the Palace of the Dukes of Berri at Poitiers and a reproduction of the miniature in the Très Riches Heures, shewing the Duke at table in his hall, surrounded by the courtiers and the retinue of his household. Such comparisons explain the apparent austerity of apartments like the Tour Maubergeon of the same Palace where the broad, flat spaces on the walls cry aloud for the rich tapestries which once adorned them. This fair civilization, courtly and aristocratic, was ruined by the Hundred Years War and the miseries which accompanied that protracted struggle. When, under Louis XI, France emerged again into more tranquil waters medieval art was already in decline. The flamboyant, which is characteristic of the last century of the Middle Ages, was, as the author recognizes, essentially sterile. The future was with the new Italian style of the Renaissance, on which she passes so harsh a judgment. C. A. RALEGH RADFORD.

RIO GRANDE GLAZE PAINT WARE; a study illustrating the place of ceramic technological analysis in archaeological research, *by* ANNA O. SHEPARD. *Contributions to American Anthropology and History*, vol. VII, no. 39. *Carnegie Institute of Washington*. 1942.

From time to time experiments have been made in the comparative analysis of pottery by spectrograph or by petrographic microscope, and also by firing tests, notably in Mrs Ehrich's *Report on the Early pottery of the Jebeleh Region* and in a study on the Iron Age pottery of Tell Beit Mersim (*A.A.S.O.R.* XXI–XXII). The results have been rather inconclusive, mainly through lack of comparative material, but due also to the expense in obtaining expert examination and advice.

An expansion and simplification of the technique of petrographic analysis has recently been carried out in America on a large scale and one may at last see the results of comparison over a wide series of material. Miss Anna Shepard's study of the Rio Grande glaze paint ware, the result of some years work on the material in the Pecos area, is based on the classification of sherds by the temper, or degraissant. The findings are most interesting both from the technological point of view and from the archaeological distribution of types.

The pottery selected for the survey was well-known over an area some 320 km. long by 160 km. wide. The material examined was from both excavated sites giving a stratified series, and from a large number of sherds collected from sites surveyed in the area. This material had already been studied typologically and stratigraphically, and the technological study was made as a check, as to the region in which the pottery was made.

The system used for the technical classification was Temper identification in a large number of sherds to provide the statistic required. 'The preparation of thin sections of pottery for analysis in polarized light requires so much time that it is not practical to identify great numbers in this way. It has been demonstrated, however, . . . that once a tempering material has been accurately identified with the petrographic microscope and its peculiarities determined, it is possible within certain limits to recognize it by examining a fresh break of the pottery with a binocular microscope . . . Identifications can be rapidly made in this way, thus making it possible to examine the entire contents of a stratigraphic test. The reliability and usefulness of the method is largely dependent upon its joint use with the petrographic microscope, and both methods were employed throughout . . .'. Over 17,000 sherds were examined and the resultant data has been summarized on maps and graphs, giving very satisfactory additional information to the archaeological results, and in some cases, making substantial corrections. Further subdivision of the pottery types and distribution was made possible by the temper designation, and in other cases, the place of manufacture was identified through the geological source of the tempering material.

The study is long and detailed, with special attention to the methods of use, the author being well aware of its limitations. However the results deserve very careful consideration by archaeologists in all parts of the world. An important factor is the use of the binocular microscope by a technician with more elementary training. Under the expert guidance of the petrologist, a large quantity of material can be economically examined with some 90 per cent accuracy. Secondly, the information gained in the production and distribution of a particular ware was most significant even when at variance with previous archaeological analysis.

This paper deserves wide study in the application of technical research on ancient pottery and is indicative of the way in which expert technical advice and co-operation can be really helpful to the archaeologist. Were this method to be applied, say, to Mycenaean pottery, one might expect some far reaching and interesting results.

J. du P.T.

MEROWINGERZEIT. *By* Gustav Behrens. (Römisch-Germanisches Zentralmuseum zu Mainz, Katalog 13; Original Altertümer des Zentralmuseums zu Mainz). *Mainz*, 1947. *pp.* 83, *figs.* 165, *pl.* 8. *No price indicated.*

It is a pleasure to welcome one of the first products of sound German archaeological work since the War. This is a Catalogue of Merovingian Antiquities in the Mainz Central Museum. Practically every piece listed is illustrated, generally by a line drawing, sometimes by a half-tone block. Except in certain instances where a fuller account is demanded, descriptions are very brief, but in conjunction with the corresponding sketch, suffice to give a clear and accurate picture of the object.

Pages 1–35 cover antiquities from the cemetery at Schwarzrheindorf—eighty-seven grave groups and a quantity of loose finds. Some of the best pieces from the grave-groups were sold to museums in Berlin, Bonn, Frankfurt-am-Main and Hamburg before the bulk of the material reached Mainz. These pieces, though not in the Mainz Museum, are also listed, so that the grave groups are given complete, in accordance with the excavator's record. The cemetery is rich in combs, beads, brooches and pins, but particularly in glass and pottery. The latter includes an unusual proportion of wide bowls, and much of it is of hard, wheel-turned light-coloured often reddish ware, having a distinctly Roman look. After the grave groups, unassociated finds from the same cemetery are dealt with by types. They include buckets, weapons, pottery and some interesting brooches, three in the form of equal-armed crosses. The description of the grave goods is preceded by some general comments on the cemetery and bibliographical references to it. After the description of the grave goods there is a list giving particulars of the pieces from the cemetery that went to other museums with their present whereabouts, an analysis of the furnishing of men's and women's graves, and some archaeological comments on the finds.

The next group of objects (pp. 35–9), from the Heerdt Collection acquired in 1932, have no provenance and are not in grave groups. The pieces themselves suggest a Middle Rhein origin. Several good bird-brooches, some pendant crosses and others for attachment, and an exact parallel (previously published in Mainzer Zeitschrift, 1933, Taf. 18, 3) to the openwork disc-pendant with ' griffins ' from Andernach in the Ashmolean Museum (E. T. Leeds, *Early Anglo-Saxon Art and Archaeology*, Pl. XVII, *c*.) may be mentioned.

The antiquities from the Fliedner Collection, purchased in 1928 (pp. 39–68) are more numerous. Weapons of all types, buckles, strap-ends, square-headed, radiate and round brooches, the latter mostly decorated with garnets or stamped-foil designs; circular openwork ornaments in geometric pattern, combs, ornamented pins and other miscellanea are well represented. The sheet-bronze ornamental mountings of a wooden casket, showing stamped and repoussé ornament of concentric circles in rectangular fields, bordered with broad bands of punched dot patterns, is particularly interesting (this piece is already published by Alfoldi, *Acta Archaeologica* V, 1934, ' Eine spätromische Helmform ' S.114, Taf. 9, 2). But the main interest of this collection lies in the wide range of ceramic types, including bottle-vases very similar to those from the Faversham cemetery, as well as handled and spouted jugs, stamped urns, and bowls ; about a hundred and twenty pottery vessels being illustrated.

The Catalogue concludes with a section devoted to miscellaneous Merovingian antiquities, most from Germany (pp. 69–81). Notable amongst these are a small pail ornamented with bronze mounts in the style of that from Beauvais (Leeds, op. cit., fig. 4) and the well-known Buire sur l'Ancre bucket (Leeds, op. cit., fig. 5) ; and an unusual handled and lidded bronze ewer (Abb. 158), paralleled at Dettingen (Veeck, die Alamannen in Württemberg, S. 30, Taf. 20, B4). The quality of the eight concluding plates is naturally enough, in view of the acute shortage of materials in Germany at the present time, not of the best, but the publication as a whole is a useful picture-book and place of reference for Middle-Rhenish antiquities of the Merovingian period, which are so closely related in many ways with finds from Kent. R. L. S. BRUCE-MITFORD.

THE NEOLITHIC PERIOD IN BULGARIA. (American School of Prehistoric Research. *Bulletin no.* 16). *By* J. H. GAUL. *Cambridge, Mass*: 1948. *pp.* XXV *and* 251. *Plates* I–LXIX.

The Bulletin contains an obituary of G. G. MacCurdy, the founder and latterly Director of the American School, and a long list of his publications; also an obituary of J. H. Gaul who spent more than a year on archaeological research in Bulgaria in 1938–9, was chased and captured by the Nazis in Slovakia and shot in 1945. By his death, at the age of 34, European Archaeology suffered a tragic loss.

The main body of the Bulletin is a general descriptive account, largely by sites, of Bulgarian Prehistoric Archaeology. The illustrations are good (though they suffer from the lamentable defect of having no scale and giving no references to the text) and the publication will be useful to all students of this important but complicated subject, as a great deal of the material has not previously been available in a W. European language.

The author first describes the W. Bulgarian Painted Culture: this, he suggests, came from the Morava Region and also up the Struma some time at the end of the 4th millennium B.C. And this, in spite of the evidence from Karanovo (pp. 20 and 44) where remains of this culture are reported from a level above the typical Mound Culture.

The largest section naturally deals with the Bulgarian Mound Culture and its predecessor Boian–A. Although he differentiates these two, the differences are not always easy to follow, and Boian–A does not apparently occur in Bulgaria except at the lower levels of the Mound Culture Sites. Boian–A he dates about 3000 B.C. and the Mound Culture 2500–1900 B.C.

Later chapters deal with the Bronze Age, but much of the material figured is more likely to be Iron Age, e.g. Thumb-grip Handles (Plates XIX.3 and LXIX.2), cf. Chauchitsa (Casson, also Heurtley). In spite of a great mass of prehistoric material, there is very little that could be referred to a Bronze Age as that term is understood in neighbouring countries, and it seems to the reviewer that the Mound Culture lasted a very long time and possibly right down to what is normally known as the Iron Age.

The only solution for these problems is further scientific excavation, and considering the geographical position of Bulgaria it is of the greatest importance.

A number of misprints occur: e.g. *re* Plates p. 36 for XI read XII; p. 55 for XLIX, XLV; p. 136 for XXVII, 3 XXV.4 and for XXV,5, XXV,3; p. 147 for XLII.5, XLII.2: p. 198 for XLVI.7,8, read XLVI.5,6.

Might one suggest, with hesitation, that the pot figured on Plate XIII from K. Dermen is medieval? The suggested parallel from Tsangli is not convincing. D. A. J. BUXTON.

MYCENAE—an Archaeological History and Guide. *By* ALAN J. B. WACE. *pp.* XVIII, 150, 110 *plates. Princeton University Press* (*London, Geoffrey Cumberlege*). *Price* $15 (*England*, 120*s*).

This magnificently produced book by the foremost living authority on Mycenae makes available the full, accurate, and detailed description of the site we have so long been in need of. The publishers state in their notice 'the volume will not only be used by students of history and archaeology, but will serve as an indispensable guide for the traveler (*sic*) at the site'. The first part of the statement is undoubtedly true, but though the traveller may, and should, study this book in the 'Inn of the Fair Helen of Menelaus', so rightly recommended by the author, before and after his visit, it may be doubted whether the size and weight of the volume will commend itself to him as a guide book to be carried in the hand when on the site itself.

This is the first full description of Mycenae in English since Frazer described it in 898 in his edition of Pausanias, and a comparison of the two shows the great advance that has been made in knowledge both of the site of Mycenae itself and of the whole of the prehistory of Greece as a result of the excavations and research of the last 50 years. It is fitting that it should be Wace, who has summarized and described these advances, many of which are the results of excavations that he has himself carried out. His name is now as indelibly associated with Mycenae as is Schliemann's.

The book begins with an evocative chapter on the topography of Mycenae in which all the author's feeling for the Greek countryside is expressed in restrained but vivid language, such that even those who have never visited Greece can feel the charm and fascination of that combination of modern scenery and past glory which is the country's special appeal.

After this description of the site there are three general chapters, on its chronology, its tomb types, and its history. In this last chapter a paragraph only is devoted to the history of the excavation. A more detailed story would have been useful here as it is only with a knowledge of the work of the various excavators from Schliemann on that the changing views on the chronology and significance of the site can be understood.

Chapter VI starts the detailed description which forms the main part of the book. Beginning with the Treasury of Atreus the reader is led round the other tombs outside the citadel and then up to the citadel itself. The descriptions are clear and admirable, but there is more than just straightforward description ; all the time the author is making new contributions to our knowledge, as in his brilliant reconstruction of the façade of the Treasury of Atreus, and in the publication of the delightful little ivory group of two women and a child found in the Palace during the 1939 excavations.

The book is not merely a dry description of architectural features. An attempt is made all the time to explain and interpret the function of the various parts of the complex and to use them to throw light on the life of the time when they were built, and in the final, all too short, chapter a summary is given of the present state of knowledge of the material culture of Mycenae. What is lacking here is perhaps any attempt to reconstruct the social relationships of the people, admittedly a difficult task in the absence of written records, but one which with the richness and variety of the archaeological remains could well be attempted. Childe with far poorer archaeological material has made a case for his theories about the social relations of the early Scots ; the scholars of Mycenaean archaeology could well attempt a similar treatment for their material.

The important problem of the date of the Treasury of Atreus is dealt with in an appendix which is substantially the same as the author's article in this journal (1940, p. 233 ff) where the case for the 14th century date as against an earlier one is carefully argued. It is a measure of Wace's contribution that this view regarded as heretical in 1926 now commands a great measure of support. P. L. SHINNIE.

LES FOUILLES PRÉHISTORIQUES. *By* A. LEROI-GOURHAN. (Technique et Méthodes), with an appendix *by* A. LAMING. *Paris* : *A. and J. Picard*, 1950.

MANUAL OF ARCHAEOLOGICAL SURVEYING. *By* A. H. DETWEILER. *New Haven* : *American Schools of Oriental Research*, 1948.

It is eighteen years since I reviewed a French manual of archaeological excavation (ANTIQUITY VI, 55), and the present booklet by M. A. Leroi-Gourhan, director of the 'centre de documentation et de recherches préhistoriques du Musée de l'Homme', is a straw that shows which way the wind has blown in the meantime. Very little wind and not much of a straw : its effective length is only 30 small pages, to which appendices and diagrams are added. As is natural in France, it betrays a preoccupation with cave-hunting, but it touches lightly, very lightly, upon a number of reasonably modern generalities, with a recurrent note of apology that disarms criticism. 'Chez nous encore, de nombreux fouilleurs usent d'une méthode plus scientifique (than formerly), mais pourtant périmée depuis longtemps à l'étranger', '. . . l'idée, déjà bien vieillie à l'étranger . . .', and the like, indicate an inferiority-complex which, in spite of obvious good intention, this booklet amply justifies. The appendix, dealing with special skills such as pollen-analysis, tree-ring analysis and even 'coal fourteen', is pathetically lacking in significant detail, and the bibliographies are remarkable alike for their omissions and their inclusions. ' Air-photography ', for example, confines its book-list to a popular French picture-book and Baradez's (excellent) recent *Fossatum Africae*, with no mention of the pioneer-work of Crawford and others and no proper reference even to Poidebard. The cultural background of the book is indeed charmingly implied in the following paragraph : ' Le moment le plus dangereux est celui d'une vraie grande découverte ; l'exaltation risque de devenir telle que le fouilleur oublie la moitié des consignes, pousse des exclamations admiratives et s'aperçoit trop tard que sa documentation est incomplète. Il est vraiment grand s'il a le courage de s'asseoir et d'allumer une cigarette pour réfléchir '—preferably, we may add, with the aid also of R. J. C. Atkinson's *Field Archaeology*, which need certainly have no fear of its new Gallic rival. Incidentally, throughout the book the *préhistorien* is scrupulously distinguished from the *archéologue*, a subtlety which escapes the understanding of the foreign reader, though it is evident that the *archéologue* is a cruder species, inclined to ' travail brutal '.

Detweiler's book is more limited in scope, though not so limited as its title implies. The author is professor of architecture at Cornell University and has participated in excavations in Palestine, Syria and Mesopotamia. His advice is therefore of an experienced and practical kind, and in so far as it is limited to actual survey it fills a need. But it is evident that, on the sites where he has operated, his operations were by no means so restricted. He includes in the surveyor's task responsibilities for which, *qua* surveyor, he has no qualification. The surveyor, he affirms, must be ready himself to ' train the workmen in the technique of exposing the building walls with a minimum of damage '. ' He is also responsible for establishing the loci of all non-architectural objects that are found '. Nevertheless, ' he will always have a great deal of surveying and drafting to do, so he must limit the time he spends supervising actual excavation. *If, however, other members of the staff can be trained to recognize and protect architectural remains as they appear, the architect can budget his time so that only occasional visits to the actual digging area are necessary* ' (my italics). What a vision of muddle all this (and more like it) conjures up ! The background is that of a typical pre-war Near Eastern excavation where stratification was ill-understood, assistants ill-trained, and supervision sketchy. Detached from this unhappy background and restricted to the proper implication of its title, the book would be a useful guide to the student in a matter where guidance is peculiarly necessary.

R.E.M.W.

GLASS AND GLAZES FROM ANCIENT EGYPT. *By* MRS ELIZABETH RIEFSTAHL. *Brooklyn Museum*, 1948. 24 *pp.*, 36 *figs.* 40 *cents.*

This is an excellent little handbook and can unhesitatingly be recommended as an elementary guide to the subject, even for those who may never see the fine Brooklyn collections.

Museum guides are not usually, nor need they be, the product of deep research, and this one is no exception ; but I know of no other place where the basic information about Egyptian glass and glazes is set out so lucidly and so effectively in non-technical language. Unfortunately, however, the illustrations, which are at times fuzzy and apparently out of focus, are not up to the standard of the text ; and it is a pity that more pictures of glass vessels were not available for inclusion, for the four small New Kingdom vessels in fig. 1—the only ones illustrated—are far from representative of dynastic Egyptian glassware as a whole.

There are a few points which should be looked into when the time comes, as it must, for a second edition. It is doubtful whether there is any truly black glass known from antiquity (p. 5). There is much glass which appears to be black, but when seen in very thin section, it proves to be really a brown metal. The grotesque face-pendants (fig. 17) are surely 5th–4th century B.C. at latest, rather than Hellenistic or Roman. On the other hand the large Aphrodite figurine (fig. 22) is not likely to be earlier than Roman, for the type resembles closely many small Roman bronze Aphrodites. The spotted lion (fig. 8) which Mrs Riefstahl will not date, would seem to be a typical Middle Kingdom product. And, finally, Lucas's article on Glazed Ware in Egypt (*J.E.A.*, XXII) should be added to the otherwise excellently eclectic bibliography. D. B. HARDEN.

BEYOND THE BOUNDS OF HISTORY : Scenes from the Old Stone Age, *by* HENRI BREUIL. English Translation *by* MARY E. BOYLE : Foreword *by* FIELD-MARSHAL J. C. SMUTS. *P. R. Gawthorn Ltd.*, 55 *Russell Square, London, W.C.* 1, 1949. 12*s* 6*d.*

This is a book of pictures of prehistoric life as it was lived by our remote ancestors for thousands of years ; it was the normal life of all mankind until quite recently, and if the child is father of the man, we must all inherit much from that childhood of our species. Many attempts have been made to reconstruct such early life pictorially, but most of them have been by persons who had little or no first-hand acquaintance with the facts. The value of these drawings lies in the fact that they are by the Abbé Breuil, who is universally acknowledged as the greatest living authority on Old Stone Age man and his works, and on the other animals amongst whom he lived. No one has ever had such a range of first-hand contact with the facts of prehistory in three continents ; and it is that which gives a unique value to his imaginative drawings, counterbalancing a certain crudeness of execution that may even have merits of its own. The author has two other qualifications—a knowledge of animals and a sense of humour. No one can sympathetically understand prehistoric life unless he has, like the author, a countryman's feeling for the animals that played so large a part in it. That feeling is not necessarily friendly, and prehistoric man's feelings were probably mixed ; it might best be described perhaps as a sense of community. (One suspects, for instance, that the modern farmer prefers his cattle to some of his urban customers).

The book is partly intended for young people—who have made several important discoveries of prehistoric man's handiwork. It is well produced ; the many scenes are reproduced in colour, and we confidently recommend the book to schools and all who want to understand what life in those ' good old times ' was really like. O.G.S.C.

THE LUNGFISH AND THE UNICORN : an excursion into romantic zoology. *By* WILLY LEY. *Hutchinson's Scientific and Technical Publications*, 1948. 254 pages. *Price* 10*s* 6*d.*

Do not be put off by the dust-cover or title of this book. It is both trustworthy and readable, and it is briefly reviewed here only because it touches no more than the fringes of archaeology. It is permeated with the idea of evolution, and the author is obviously familiar also with the history of zoology—which is one reason why it is so readable. Another is the author's style which, in spite of occasional lapses, is simple and direct. Unicorns are fair game for archaeologists, and it does seem that Mr Ley has run the myth to earth—in a mistranslation in the Bible. We particularly relish the story (of Jewish origin) that it was too big to go in the ark and had to spend the time swimming about round the Ark occasionally resting the tip of its horn thereon—swimming on its back no doubt, as we do when resting in the water.

Chapter XIV, the story of Gondwanaland, would have been improved by a sketch-map showing the extent of that lost continent (or a series of such, showing its gradual break-up) : which leads to the suggestion that Mr Ley should write another book dealing on zoological lines with palaeogeography—marginal survival and that sort of thing, not omitting man himself. O.G.S.C.

Antiquity

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Editorial Notes

DATING the past in years, and not merely in periods, is the aim of archaeology. The C 14 method just discovered will probably be used to date all sites in future, but its range extends only to about 25,000 B.C., and man existed long before that. Now comes a new technique which promises to carry the story back beyond the limits of C 14. It is based upon the fact that the deep ocean deposits still forming have been forming continuously over a very long period of time. They are of course stratified, and by making a boring through them and analyzing the contents of the core, certain very mportant conclusions can be drawn about the climatic changes that took place during and after the Ice Age. These agree quite well with what was already known, and will enable us to give dates in years—i.e. an absolute chronology—to the phases of the Old Stone Age. We hope shortly to publish an article on this subject. Meanwhile we quote (with the permission of the Editor and Author) from an article which appeared in *The Times*, 22 Sept. 1950, p. 5. It is by Dr J. D. H. Wiseman, of the British Museum (Natural History). ' As the distance from the top of the core of the sample corresponding with the climatic optimum is known, it is possible to calculate with a considerable degree of accuracy the age of any portion of the cores, provided the distribution of calcium carbonate as well as the original water content is known. It is in this way possible to assign dates to climatic oscillations. About 117,000 B.C. there was a distinct but sharp minimum temperature. Thereafter the temperature rapidly rose, and with some minor oscillations reached a maximum at 78,000 B.C. The temperature then fell to a minimum at 68,000 B.C. which was not as cold as the minimum at 117,000 B.C. After that the temperature increased until 43,000 B.C.; it then remained essentially constant for 15,000 years. There was a distinct fall in temperature and at 23,000 B.C. there was a minimum which was not as cold as the two preceding minima. The temperature then increased, remained for some time essentially constant, and at 10,000 B.C. there was an indistinct minimum. After that the temperature rose rapidly until 7,000 B.C., when there was a more gradual rise until the climatic optimum at 3,000 B.C. was reached. These dates are approximate, as further investigations might lead to minor modifications'.

* * *

Every year the British Association for the Advancement of Science publishes a brochure containing the presidential addresses delivered to its thirteen sections. They provide an admirable conspectus of the general state of knowledge in the field of science, and though unfortunately there are always some presidents who lack the powers of lucid expression, they make good reading on the whole. The address delivered to Section L

(Education) by Mr W. F. Oakeshott, headmaster of Winchester College, is short but stimulating. If we have understood him correctly, he laments the cleavage which began (as he thinks) soon after the middle of the 19th century, when a general acquaintance with the state of scientific knowledge gradually ceased to be part of the normal educated person's mental equipment. 'The educated man of the 18th century still regarded the discoveries of the scientist as being part of his normal reading'; and Darwin's *Origin of Species* and Lyell's *Principles of Geology* were 'widely read by men who were not primarily scientists'. The reason alleged for this cleavage is that the specialist no longer feels an obligation to write books for the layman because that service is now performed by others. Archaeology is cited as an example. 'What is wanted is that the expert archaeologist ... should write books for the general public'. We rubbed our eyes in amazement when we came to this sentence. Has Mr Oakeshott, who calls himself an 'interested amateur' of archaeology, never heard of the many books written during the last two or three decades by 'expert archaeologists', some of them by a distinguished Wykehamist? Is there any 'expert archaeologist' in this country, indeed, who has not written at least one such book? Has he never heard of the Penguins and Pelicans, and that some of them are prescribed as standard advanced text-books in American universities? Has he never heard of ANTIQUITY? Nor does his accusation hold against specialists in other branches of science; there have been many such books published. The fault lies not with the experts but with educators who fail to use their books or even, as it seems, to know that such exist.

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What is the result? The higher posts in the Civil Service are filled with people who are ignorant of the most vital knowledge of our age, and consequently out of sympathy with that spirit of research which animates and is the driving force of many lives. We may be too optimistic, but we cannot believe that bureaucrats are all as bad as Mr Oakeshott's former chief who (he says) enjoyed 'the power [his post] gave him of doing harm to better men than he was'. We have all met people of this kind, but we cling desperately to the theory that ignorance rather than mere love of power is responsible. In fact, like Mr Oakeshott, we prefer St. Benedict's view of human nature to that of Machiavelli. Ignorance not malice is surely responsible for most of the misunderstandings that arise between the world of learning and science on the one hand and that of politicians and administrators on the other. For that ignorance our educational system must bear part of the blame; but the remedy is not easily found, for that system is controlled in the last resort by its own products—or victims.

The Zürich Congress*

by Stuart Piggott

University of Edinburgh

WHEN the Editor of ANTIQUITY invited me to write some notes on the Zürich Congress, it was agreed that they could only be considered as personal impressions, and not a formal report. The unfortunate human limitation which debars one from being in two places at once is always irritatingly obtrusive on such occasions, and a decision on which of several simultaneous papers to attend is likely to be decided by spinning a coin at the last moment, so that one can do no more than record one's own impressions, and something of those of friends to whom one has talked.

In general, I think we all carried away a feeling that the Congress had been a great success, with an admirable blend of hard work and informal enjoyment; the arrangements made by our Swiss hosts were altogether excellent in their unobtrusive efficiency. About 250 individuals had enrolled as members of the Congress, and most were present, and their proportion by countries reflects the post-war state of European archaeology—17 from the Scandinavian countries, 26 from Germany, and nine from Austria; 24 English members, with six from Eire; Holland with 17, France with 16 and Italy with 14 members. Spain and Belgium were each represented by five members, and the United States by eight, and the remaining countries represented by two or more delegates. Mr G. M. Young once defined a university in a sentence which began 'A University is a place where young men and women educate one another by conversation,' and this phrase may equally well apply to a Congress—it is the informal conversations over a drink that are often the most valuable part, and at Zürich there was plenty of such discussion, in which language barriers were always somehow overcome even by the most embarrassed monoglots.

When one comes to consider the interests of the members as indicated by the papers read, one fact immediately stands out. Palaeolithic studies, which with the addition of a few Mesolithic contributions, were represented by 25 papers, are becoming, or have become, a closed shop for most of the European prehistorians whose concern lies in this field. This is the French tradition, perpetuated indeed in the title of the Congress, whereby a *préhistorien*, concerned with the palaeolithic and (grudgingly) with the mesolithic periods, is sharply differentiated from the student of *protohistoire*, whose province stretches from the neolithic to the Dark Ages; in one otherwise excellently arranged large provincial French museum I found this summer that the neolithic material (stones, therefore *préhistoire*) was in a room remote from the protohistoric collections covering the Bronze Age to the Migration Period. At Zürich, protohistorians could be seen briskly moving between the 3rd millennium B.C. and the 6th century A.D., but the prehistorians almost to a man remained in their palaeolithic seclusion. In England this artificial distinction has never been formed, and one of the most outstanding papers given to the *préhistoriens* was the report on the Seamer mesolithic site by Dr Grahame Clark.

The main weight of papers however, exclusive of the palaeolithic series, lay in the European Bronze Age, with 22 papers as against 11 dealing with the Neolithic, 12 with the Early Iron Age, and 15 with the Dark Ages. In the Neolithic section, Professor O'Riordain gave a stimulating paper based on his recent Lough Gur excavations, Dr Mathiassen presented the new views current on the beginnings of the Danish neolithic cultures, and Dr von Gonzenbach summarized her recently published classification of the Cortaillod culture of Switzerland (1). Professor von Merhart's paper on Late Bronze Age chronology was a brilliant *tour de force*, turning largely on the controversial questions of the bronze vessels discussed in the previous communication by Professor Childe: the Late Bronze Age was again brought under review in papers by Professors Sprockhoff, Hawkes, and Kimmig and by Mr Cowen. Questions of Early and Middle Bronze Age chronology were dealt with in papers by Professor Schaeffer, M. Giot, and the writer. Schaeffer reiterated his case for a high dating of the Central European Early Bronze Age, based on Oriental types of pin and 'ingot-torque'; the chronology of the 'Wessex Culture' was reviewed in the light of recent work (particularly that of de Navarro), (2) and M. Giot presented the first up-to-date account of the Breton Bronze Age since du Chatellier's day.

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* The Third International Congress of Prehistoric and Protohistoric Sciences, Zürich, 14–19 August, 1950.

In the Early Iron Age section, the most controversial communication was that of Dr Raftery, claiming a La Tène date for Cairn H at Loughcrew (and by inference other chambered cairns of the same type) in the light of his excavations there: judgment on this bold claim must obviously he reserved until the publication of the full report. In the Dark Ages, important papers were read by Professor Werner, and Mr Bruce-Mitford displayed the wonders of Sutton Hoo.

Professor Vogt's public lecture on the problems of the Swiss Neolithic and Bronze Age was, as might be expected, a notable contribution, but one paper of great general interest needs special mention—that of Professor Braidwood on Carbon 14 dating in relation to Oriental prehistory. He began by describing in outline the theory of radioactive carbon dating, now familiar to readers of ANTIQUITY, and then gave the results of recent examination of organic meterials from certain Near Eastern sites. His own recent excavations at Jarmo (3), in the Iraq hills, have brought to light a settlement of people who were agriculturalists, growing wheat and barley and domesticating animals. Though clay-lined hearths were present, pottery appears to be unknown, though there was an industry in stone, chipped, flaked or polished, and unbaked clay figurines. Such a culture should, on general grounds, be early on the Mesopotamian sequence, and possibly antedate the beginnings of Hassuna, so that the Carbon 14 dates might be expected to be high. The figures were in fact 6890±320 years Before Present, giving a corrected date of *c.* 5260–4620 B.C.—a by no means fabulous antiquity. In this connection further C 14 dates are interesting—Fayum A, with dates of 6219/6304±330 B.P., or *c.* 4784–3929 B.C.; El Omoni, *c.* 3680–3220 B.C., and Alishar Chalcolithic, *c.* 2950–2450 B.C. Material from Hemaka, dated archaeologically to the middle of the First Dynasty, gave a date of *c.* 3397–2734 B.C. Professor Braidwood stressed the fact that these dates are still regarded by the physicists as highly provisional, but at all events they are a beginning. One hopes that other determinations will be made available before long, and especially, in the Near Eastern context, that there may be Natufian material that could be used for C 14 examination.

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[1] *Die Cortaillodkultur in der Schweiz* (Basel, 1950).

[2] 'The British Isles and the Beginning of the Northern Early Bronze Age,' in *Early Cultures of N.W. Europe* (Chadwick Essays, 1950).

[3] See his article in this number, pp. 189–95.

Apart from the papers read at the Congress, attention should be drawn to the excellent film, made by M. France-Lanord in the technical laboratory of the museum of Nancy, demonstrating the investigation of pattern-welding in Dark Ages sword-blades. This was exhibited by M. Salin, and raised the hopes of many of us that further investigations should be carried out in this country, not only on Dark Ages weapons (4), but on Early Iron Age swords as well. Professor Vogt had also arranged a special exhibition of drawings to illustrate current Swiss technique in the recording of plans and sections in archaeological excavations, and their subsequent preparation for publication. As might be expected, the standard was very high, the working drawings being usually executed in coloured pencils and, in the case of the sections in particular, being drawn in great detail in an attempt to achieve fidelity in copying each variation of tone and colour in the soil. The whole question of representation and convention in archaeological section-drawing is one which deserves full-length discussion, but the direct half-tone reproductions of some of the Swiss drawings seemed to be very unsatisfactory as illustrations to the excavation reports, and one felt that some of the records—for instance a plan of a group of post-holes in which every natural crack and crevice in an expanse of rock surface many square metres in area had been laboriously drawn—were really unnecessarily elaborate and added nothing to the value of the diagram. After all, any archaeological draughtsmanship is a matter of working to a set of black-and-white conventions, and an attempt at producing an allegedly completely objective and accurate record of the appearance of the soil is bound to fail. Here, as in all aspects of an excavation, the excavator's responsibility does not end with pure record, but must always include the interpretation which he alone, working day after day in the closest contact with the site, can properly apply to its problems.

A mid-week excursion was made from Zürich to visit the palaeolithic rock-shelters of Schweizerbild and Kesslerloch, dug in the last century, and also to a most interesting new excavation, made specially for the Congress, on the well-known 'lake-dwelling' site at Weiher, of the Michelsberg culture. The area exposed included vertical piles and a section of a corduroy road, while the presence of a tree-stump *in situ* implied, as Dr Bersu pointed out on the spot, that the settlement had not been raised above the water, but was actually founded on more or less dry land—the site in fact must have been on an island in a lake formerly filling the little valley, now dry and cultivated. A similar demonstration excavation was also visited at the Ile Werd near Stein-am-Rhein, where a good stratification of neolithic and Schnurkeramik levels was exposed. After the formal Congress week, the new excavations at the classic site of Auvernier, on Lake Neuchâtel, were visited, where M. Perret is testing the stratification established by Vouga, and uncovering a considerable area of the earlier Cortaillod level—a very fine wooden knife-handle is one of the most striking finds from this layer. The results of this excavation, taken in conjunction with those of M. Leroi-Gourhan's test dig at the same site (5), should do much to clarify our knowledge of the culture-sequence at Neuchâtel.

In addition to the official programme, the Congress published a very valuable *Guide d'Excursion*, with notes, bibliographies and illustrations of the sites visited after the Congress, and the Schweizerischen Gesellschaft für Urgeschichte dedicated a special volume of its *Jahrbuch* (Vol. XI, for 1949–50) to the Congress, containing a most valuable series of papers on the archaeology of Europe and beyond : Professor Childe's study of the contrasted areas of use of axe and adze, bow and sling, in prehistory, dealing with the

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[4] Cf. Mr Herbert Maryon's work in *Camb. Ant. Soc. Proc.*, XLI, 73.

[5] Preliminary report in *Ur-Schweiz* XIII (1949), 1–6.

The Antiquity of *t'ou hu*

by Richard C. Rudolph

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SOME ten years ago Professor G. Montell published a well-illustrated and highly interesting account of the history of the Chinese game of *t'ou hu*[a] or Pitch-pot (1). In this game, practised in China for many centuries, two contestants attempt to toss (*t'ou*) arrows into the mouth of a narrow necked vessel (*hu*) according to very elaborate rules of procedure. He began by quoting verbatim Legge's translation of the lengthy description of the game as contained in the *Li chi*,[b] (2) taking this as evidence that the game 'was perfected as early as the Chou period (3) '. He surmised that the game survived the centuries although he knew of ' no further mention of it in the literature until the time of the Sung dynasty (4) ', his next source dating from the 11th century (5).

Two 2nd century illustrations of the game, and various literary references prior to the Sung dynasty (960–1280) having come to my attention, I offer this additional material as a supplement to Professor Montell's illuminating article.

The game of pitch-pot undoubtedly existed as early as the Chou dynasty, but the text of the *Li chi* that has been handed down can hardly be considered as the most reliable evidence for this. It is now well known that although the *Li chi* purports to describe ceremony as it existed during the latter Chou period (1122–255 B.C.), it actually was written in the Early Han (206–25 B.C.) dynasty. It is, indeed, better evidence than none for the Chou period, and one should remember that earlier documents were undoubtedly used in its compilation. What is perhaps more reliable evidence for the existence of this game during the Chou period can be found in the *Tso chuan*.[f] There, under the twelfth year of Duke Chao which corresponds to 530 B.C., we are told that a noble on a mission to the State of Chin indulged in the game of *t'ou hu* or pitch-pot. It is possible that here again the writing may not be contemporary with the alleged event, but nevertheless most scholars who have investigated the authenticity of the *Tso chuan* agree that it was written in the latter years of the Chou dynasty. Moreover, it is generally believed that the compiler had access to the court archives of the various feudal states and it is probable that in this case a contemporary record was used. It hardly seems likely that the compiler of a historical account would go so far as to interpolate such a passage. The text is as follows :—

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[1] ' T'ou Hu—The Ancient Chinese Pitch-pot Game ', *Ethnos*, V (1940), 70–83.

[2] *The Sacred Books of the East*, XXVIII (Oxford, 1885), 397–401. Cf. S. Couvreur, *Li Ki ou mémoires sur les bienséances et les cérémonies*, II (Ho Kien Fou, 1913), 591–9. A parallel text from the related *Ta-tai li-chi*,[c] ch. 78, has been translated by R. Wilhelm in his *Das Buch der Sitte des älteren und jüngeren Dai* (Jena, 1930, 333–6. Wilhelm comments that this section is based upon earlier sources.

[3] Montell, op. cit., p. 74.

[4] Ibid.

[5] This is the *T'ou-hu hsin-ko* by Ssu-ma Kuang.[d] Cf. the *T'ou-hu i-chieh* by Wang T'i,[e] a 16th century work on the game.

> 'While the prince of Chin, attended by Chung-hang Mu-tzu, was entertaining the prince of Ch'i, pitch-pot was played. The prince of Chin was first and Mu-tzu said to him, " You have liquor equal to (the volume of) the Huai River, you have meat equal to a hillock. If my master succeed in this, he shall be leader of the princes ". He did succeed and the prince of Ch'i raised an arrow and said, " I have liquor equal to (the volume of) the Sheng River, I have flesh equal to a mound. If I succeed in this, I shall rise up and take your place ". He also hit the mark and Po-hsia said to Mu-tzu, " You erred in your speech. We are certainly the leader of the princes. That pot—why did you use it ? How should a successful throw into it give any superiority ? . . . " (6) '.

Next in chronological order comes a reference to pitch-pot in the *Shih chi*.[g] This remarkable historical work was compiled in the 1st century B.C. from reliable court archives. Although some embroidering was done by later writers, this is mainly limited to fictional conversations and flattering descriptions of events or persons. The *Shih chi* reference to the game occurs in chapter 126, that devoted to biographies of famous wits. Pitch-pot is mentioned among the pastimes of party-loving Shun-yü k'un[h] who is supposed to have lived in the early part of the 4th century B.C. Whether this is legitimate 4th century evidence of pitch-pot is debatable ; but its authenticity as a first century B.C. reference to the game cannot be denied.

Another reliable early source that mentions the game is the *Hou-han shu*,[i] the *History of the Late Han Dynasty*. Although put in its final form in the 5th century, it is undoubtedly compiled from contemporary Han records. In the biography of Chai Tsun,[j] (7) a military leader and idealist who flourished during the 1st century, we are told that he laid great emphasis upon the game of *t'ou hu*. Li Hsien,[k] the 7th century commentator on this history, refers to the *Li chi* for information on *t'ou hu*. This suggests that no other works on the game were extant in his time.

The above sources are only a few of some twenty pre-Sung literary references to *t'ou hu* that I have seen in various texts and reference works. Let this suffice for the texts while we turn to actual pictures of the game as it was played in the 2nd century.

Unquestionable pictorial representations of the game are depicted in two stone reliefs dating, at the latest, from the 2nd century A.D. Both of these reliefs were decorations on Late Han stone tombs discovered some years ago around Nan-yang in Honan province (8). Figure 1 shows a clear and unmistakable representation of two kneeling men playing at the game of pitch-pot. Each one holds arrows over one shoulder and tosses an arrow at the vase which is between them. One should expect both men to be holding their supply of arrows with their left hand and tossing with their right, but in this relief the artist, perhaps for the sake of symmetry, shows one man holding his arrows in the above manner, and the other holding his arrows with his right hand while he tosses with the left. Each man seems to have been supplied with five arrows ; there are two in

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[6] Cf. James Legge, *The Chinese Classics*, vol. v, *The Ch'un Ts'eu with the Tso chuen* (Hong Kong, 1872), 639. My wording differs.

[7] *Hou-han shu*, ch. 150.

[8] *Nan-yang han hua-hsiang hui-ts'un* (Nanking, 1937), 66b, 67a. An earlier work on the Nan-yang reliefs, *Nan-yang han hua-hsiang chi*[m] (Shanghai, 1930), does not contain these scenes.

Although the rich Han reliefs from Shantung published by Chavannes and others, and the virtually unknown Han reliefs from Szechwan (soon to be published by this writer) contain numerous scenes of pastimes and entertainment, none show this game. To my knowledge, the Nan-yang reliefs provide the only illustrations in Chinese art of this game prior to the 11th century.

the vase and each contestant still has four in his possession. According to the *Li chi*, four arrows were used by each person. Beside the vase is a vessel with a ladle. In back of the man on the right is another person who is probably an attendant whose duty it was, according to the account in the *Li chi*, to keep tally. This scene is three-quarters of the picture depicted on the entire slab which measures 128 by 34 centimetres. The portion of the scene not shown here consists of two people who have no relation to the game. Figure 2, one-half of the other Nan-yang tomb relief, is much less distinct but undoubtedly represents the same game. The original was approximately the same size as the other. As in the other figure, to the left of the pot is seen a vessel on a stand.

a 投壺
b 禮記
c 大戴禮記
d 投壺新格, 司馬光
e 投壺儀節, 汪禔
f 左傳
g 史記
h 淳于髡
i 後漢書
j 祭遵
k 李賢
l 南陽漢畫象彙存
m 南陽漢畫象集
n 容庚, 適周彝器通考
o 雙劍誃吉金圖錄

KEY TO TRANSLITERATIONS IN THIS ARTICLE

Aside from their over-all value as pictorial evidence of the game in the 1st or 2nd century, the reliefs provide other specific information of interest: Examples of bronze vases for this game seen by the writer in the Royal Ontario Museum (Toronto) differ considerably from those shown in the rubbings of the Nan-yang reliefs. The examples referred to, dating from the 17th or 18th centuries, did bear some resemblance to the vase in Fig. 1 in that they had long, narrow necks and round, squat bodies. But they differed from the vases in both illustrations in that they had vertical tubes or rings attached to either side of the neck. Both the 11th and 16th century works referred to in Note 5 provide the vase or *hu* with these rings at which arrows were also aimed. The detailed account in the *Li chi*, quoted by Montell, does not mention such rings or tubes and in this respect agrees with the Nan-yang reliefs of the 2nd century. If these reliefs illustrate 'standard' equipment for the game, then the tubes, or 'ears' as they are called in the Chinese texts, were added sometime between the 3rd and 11th centuries.

But *hu* with 'ears' were made in China as early as 12th or 13th centuries B.C., although they were eliminated by Han times. Thus the use of 'ears' on the vases designed for the later versions of the game may have been inspired by very early examples (**9**),

Jung Keng in his discussion of the *hu* (**10**), although he quotes the *Li chi*, does not allude to its use in this game. Since *t'ou hu* apparently was conducted in a ritualistic manner, it would not be surprising if bronze *hu* were cast for this specific purpose.

To summarize the new data on the Chinese game of *t'ou hu* that is presented here, we may say that on the whole they substantiate Professor Montell's conclusions, that the literary evidence adds conclusive proof that the game existed in the Chou dynasty and was practised through the following centuries, and that the 2nd century reliefs are in general accord with the description of the game written some four centuries earlier but differ from later accounts.

Postscriptum : Some time after I wrote the above article, Professor L. C. Goodrich of Columbia University kindly drew my attention to the illustration of a group of Han dynasty tomb figures shown here as FIG. 3. This was described in the *Illustrated London News* (**11**) as probably being a representation of the game of mah-jong, an impossibility because this game is of much more recent origin—probably around the 11th century. This group of figures, probably dating from the 1st century, consists of two players on either side of a gaming board with an attendant in the background. On the board are five or six wand- or arrow-like objects and a large vase. The Han dynasty game of *liu po* (**12**) comes to mind in connection with six such objects and a gaming board, but the presence of the large pot eliminates this possibility and forces one to conclude that this group is intended to show a game of *t'ou hu* in progress thus supplementing the two reliefs by showing in the round the same action that they portray in profile. The positions of the hands of the players indicate that they originally held wooden arrows.

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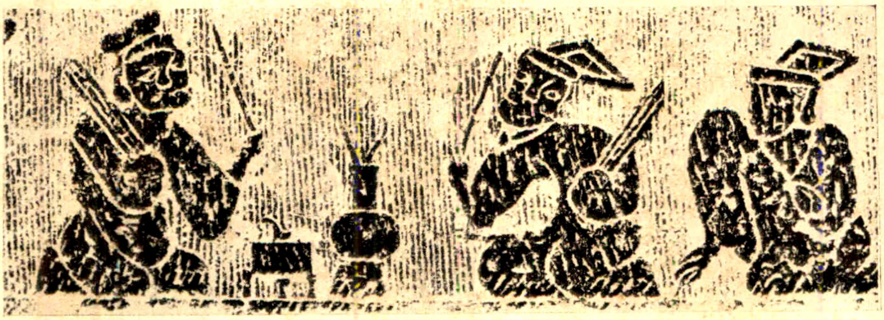
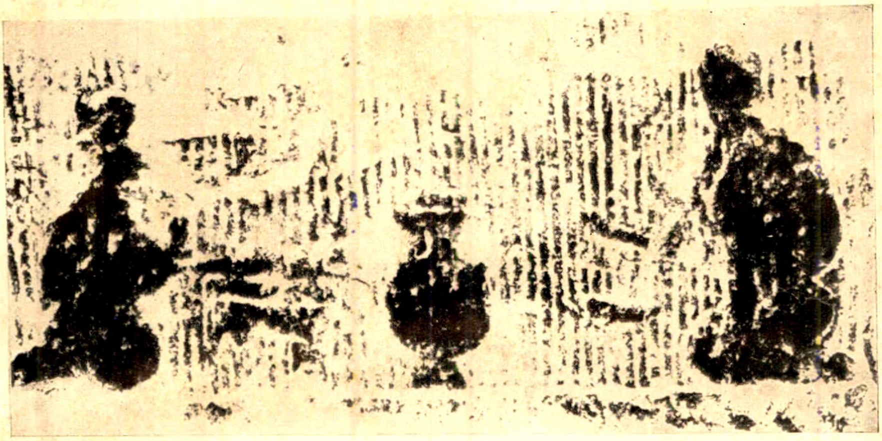
[9] Jung Keng's monumental work on ancient Chinese bronzes shows a number of very early *hu* with vertical tubes on the neck, probably used for slinging the vessel. His illustrations also show the change from tubes to free swinging ring handles prior to the Han dynasty. See Jung Keng, *Shang-chou i-ch'i t'ung-k'ao*[n] (Peiping, 1944), II, nos. 704–83. Bronze vessels of the *hu* type were commonly inscribed but I have looked in vain through collections of inscriptions for reference to the game. Several Chinese works of Han bronzes contain illustrations bearing some resemblance to the two vases shown in our reliefs. One *hu* in particular bears a striking similarity to the one in Fig. 2. See *Shuang-chien-ch'ih chi-chin t'u-lu*,[o] II, 54. See also Charles Fabens Kelley and Ch'en Meng-chia, *Chinese Bronzes from the Buckingham Collection* (Chicago, 1946), pl. LXII, LXXV.

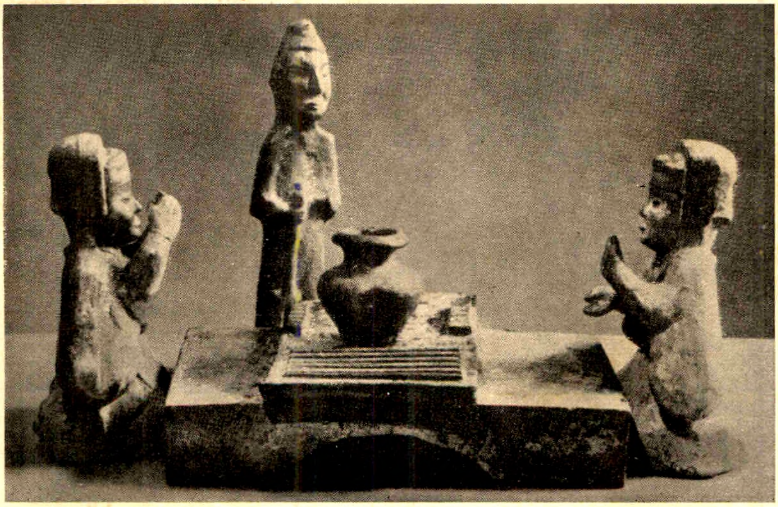
[10] op. cit., I, 432–5.

[11] 13 May 1933, p. 694, fig. 5.

[12] This game seems to have been widely played during the Han period but early Chinese literature provides but meagre information on it. See Liang-sheng Yang, 'A Note on the so-called TLV Mirrors and the Game Liu-po', *Harvard Journal of Asiatic Studies*, 9 (1945–47), 202–206.







‘Ape-Men’ of South Africa

by W. E. Le Gros Clark
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In 1925, Professor Dart of the Witwatersrand University described the well-known Taungs skull (*Australopithecus*). He pointed out that, in spite of the obviously ape-like proportions of the brain, there were certain features (particularly in the dentition) in which this fossil primate resembled Man more closely than do any of the known anthropoid apes. In general, other anatomists agreed that this certainly was so, but some regarded the resemblances as little more than interesting examples of parallelism having no particular reference to the origin of the human family (Hominidae). In 1936 and the following years, Dr R. Broom, F.R.S., of the Transvaal Museum, discovered many more remains of similar ape-like creatures, including teeth, portions of skulls and jaws, and parts of the limb skeleton. Incidentally, he believed these remains to represent two types closely related to *Australopithecus* but generically distinct, and he gave them the names *Plesianthropus* and *Paranthropus*. Whether this generic distinction will be accepted as valid is a subject of controversy. Actually, however, it is (for the moment) a matter of no great importance, for it is agreed that they all certainly belong to the same group, the Australopithecinae. Dr Broom’s account of his discoveries (which were made at Sterkfontein and Kromdraai—not far from Johannesburg) was published in 1946 (1). It became clear from his description and illustrations that the hominid traits of the Australopithecinae were even more remarkable than many anatomists had supposed. So much was this the case, indeed, that some of those who had shown a cautious reserve towards the Taungs skull now expressed their conviction that these extinct primates were by no means merely ‘ anthropoid apes ’. On the contrary, they agreed that they provided evidence of the utmost importance for problems of human evolution. But although this now became widely accepted, one or two critics continued to combat it with rather unusual (and also rather puzzling) vehemence. Meanwhile, unperturbed by these minor protests regarding his earlier discoveries, Dr Broom was quietly working away at his excavations at Sterkfontein, and in 1947 he came upon an astonishingly rich collection of Australopithecine remains closely packed together in a small and circumscribed locus of dense limestone. This new material proved to be much more complete and perfectly preserved than anything hitherto found in the area, and included several skulls or parts of skulls, many teeth, and an exceptionally fine specimen of a pelvic bone.

A monograph on the Australopithecine remains discovered at Sterkfontein since the beginning of 1947 has now been published as a memoir of the Transvaal Museum, Pretoria (2). Dr Broom and his assistant, Mr J. T. Robinson, deal with the skulls, jaws, teeth and limb bones, while Dr Schepers contributes an account of the endocranial casts of three of the skulls. The description of the skeletal material is straightforward and comprehensive, but is not intended to be in any way exhaustive. The authors are concerned mainly to give the facts. But in their introduction they permit themselves

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[1] R. Broom and G. W. H. Schepers. ‘The South African Fossil Ape-men’, *The Australopithecinae*, Transvaal Mus. Mem., No. 2, 1946.

[2] R. Broom, J. T. Robinson and G. W. H. Schepers. ‘The Sterkfontein Ape-man’, *Plesianthropus*, Transvaal Mus. Mem., No. 4, 1950.

the mild remark that, in addition to the facts, ' we must be allowed to give what are our own very definite opinions '. This, of course, is very right and proper, though it is perhaps hardly surprising if Dr Broom supposes that some of his earlier critics may have thought otherwise. No doubt the most important specimens of the new Sterkfontein material are the practically complete and undistorted skull (Sterkfontein skull No.5), and the almost complete pelvic bone. The skull is a truly remarkable find. One of the most impressive illustrations (among many which are noteworthy for their excellence) is the photograph of Dr Broom inspecting it when it was first discovered. This photograph is particularly useful since it serves to emphasize the great density of stalagmitic matrix in which all these fossils were encased. There is no question here of the fossil remains (which, incidentally, show not the slightest evidence of ' rolling ') being scattered in a loose sedimentary deposit which might have allowed the chance intrusion of extraneous material to form a heterogeneous mixture of fossils of different geological age. It has only been possible to expose the fossil specimens by blasting the dense rock in which they were sealed up, and it was a happy chance that, on 18 April 1947, a blast charge detached a lump of limestone with the roof of a skull, leaving the remainder intact and still embedded in the parent rock. Dr Broom, with consummate skill, was able to extract the whole skull from its matrix, and found that the calvarial roof fitted quite accurately in position. The importance of this skull lies in its completeness and good preservation. It thus allows a more accurate comparative study of the Australopithecine skull than has hitherto been possible.

But apart from such fine specimens as this, the great quantity of material which Dr Broom has made available by his energetic excavations has this most important result—it now enables us to take a comprehensive view of the total morphological pattern presented by the Australopithecinae, with no further excuse for relying for the assessment of affinities on individual characters of individual skeletal elements treated as isolated abstractions. In fact, the need for a synoptic study of the *whole* morphology of these extinct primates has lately become very obvious, for some of the statistical studies limited to individual metrical abstractions (without any attempt to view the situation as a whole) have evidently led to quite anomalous conclusions.

So far as the skull is concerned, there are certainly a number of individual hominid features (as I have pointed out elsewhere (3)) which do not appear to find an exact parallel in any of the known apes, and others which may occasionally be found *singly* in an ape's skull if diligently sought in a large enough series. However, in a comparative study reliance is not necessarily to be placed on any one of these features taken by itself, but rather on the *total pattern which they present in a particular combination.*

Among the approximately hominid features, which are to be found in the Australopithecine skull, are the relatively low position of the area for the attachment of the neck muscles, the relatively forward position of the occipital condyles, the height of the skull above the level of the supra-orbital margin, the construction of the region of the temporo-mandibular joint, the sharp undercut margin of the lesser wing of the sphenoid bone, the contour of the palate and the body of the mandible, the relative height of the temporal squama, and so forth. It is particularly to be noted that most of these hominid features have now been demonstrated to be *consistently* present in several specimens. For example the forward position of the condyles is shown consistently in the five skull bases available, the low position of the nuchal area in the four specimens in which the occipital region is

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[3] ' New palaeontological evidence bearing on the evolution of the Hominoidea '. *Quart. J. Geol. Soc., London*, vol. 105, p. 225, 1950.

preserved, and so forth. Thus, in order seriously to controvert the view that the Australopithecine skull as a whole makes a closer morphological approach to the hominid type of

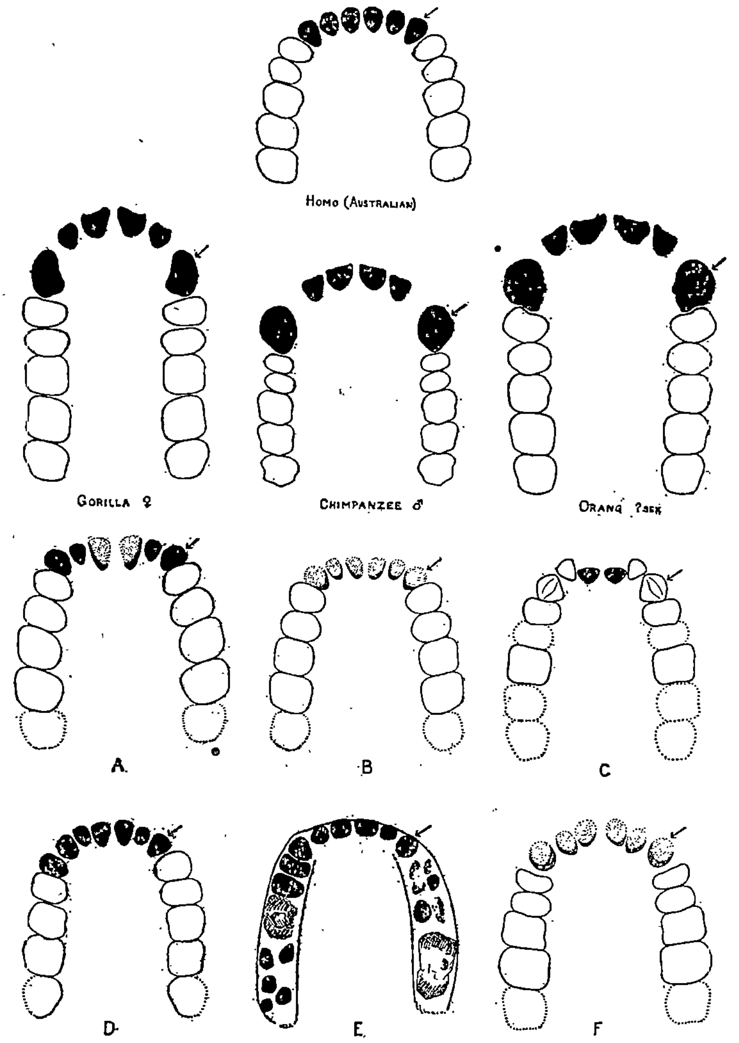

Fig. 1. THE UPPER DENTAL ARCADE OF *HOMO SAPIENS* (AUSTRALIAN NATIVE) COMPARED WITH ANTHROPOID APES (GORILLA, CHIMPANZEE AND ORANG) AND THE AUSTRALOPITHECINAE (A–F).

All at two-fifths natural size. The teeth are represented in outline, and sockets in solid black. Where any reconstruction has been necessary, this is indicated by dotted lines or stippling. Because in the fossil specimens the incisor and canine teeth are in most cases missing, they have been represented by their sockets only in the modern specimens. Note, particularly, the position and relative size of the canine or its socket (indicated by an arrow). Of the Australopithecine specimens four were found at Sterkfontein (B—type specimen, C—1936 maxilla, D—skull no. 6, and E—skull no. 5) one (A) at Kromdraai and one (F) at Makapansgat

skull, it must be demonstrated not only that a similar total pattern of hominid features may also occur in a *single* skull of one of the anthropoid apes, but also that this pattern is consistently found in several skulls taken at random from a common group (in order to parallel the morphological consistency shown by the several Australopithecine skulls which have now been found). With all the new material now collected by Dr Broom, this now appears to be no longer in question. And even if it were so, it would of course not be legitimate to consider the skull without reference to the dentition (or *vice versa*), and the dentition tells the same story.

The Australopithecine dentition (like the skull) displays certain hominid features which severally appear to find no precise parallel in any of the known apes (recent or extinct), and others which may very occasionally be found singly if sought in a large series. But, again, conclusions regarding the hominid nature of the dentition must ultimately be based on the total pattern determined by all these features together. Among the features referred to are the small size and spatulate form of the canine (combined with its flat wear at the tip by attrition with the opposite canine, and its orientation in the tooth row), the non-sectorial bicuspid form of the front lower premolar (with cusps of approximately equal bulk and height and well-developed anterior and posterior foveae), the evenly curved parabolic contour of the dental arcade with the *consistent* absence of any diastema (see FIG. 1), the flat type of wear of the premolars and molars in the earlier stages of attrition, and, in the temporary dentition, the shape and dimensions of the milk canine combined with the very hominid cusp pattern of the first milk molar. Dr Broom and Professor Dart have now brought to light many specimens of jaws and palates of the Australopithecinae, as well as numerous isolated teeth, and at least three excellent specimens of the milk dentition are also known. All this amazingly rich material bears witness to the fact that the same hominid pattern is quite consistently present in the Australopithecine dentition. By contrast, no similar combination of these hominid features together has been demonstrated in the dentition of any of the anthropoid apes. The implications of these observations seem clear—that in their dentition the *Australopithecinae* display a much closer resemblance to man than do any of the known anthropoid apes. Yet, in reference to a recent statistical study by Ashton and Zuckerman, the remarkable statement (4) has been made that of 75 dental characters of *Plesianthropus* none differs from the orang! As it is phrased, this statement may have the unfortunate result of leading the casual or uncritical reader unwittingly to suppose that an exhaustive statistical study has finally shown that *Plesianthropus* in its dentition is indistinguishable from an orang. This however, is certainly not so. In the first place, it has to be decided whether the characters referred to are really relevant to this particular problem of taxonomic affinities. But the fundamental question, surely, is whether the authors can produce for inspection even one sample of the dentition of an orang or other anthropoid age (let alone a whole series of individual specimens) in which the total hominid pattern consistently found in the Australopithecine dentition is reproduced, or even closely approached. Failure to do so must (it seems) render highly suspect a method of approach which, at first sight, appears to lead to conclusions so greatly at variance with the much more complete evidence which can be provided by the method of direct visual comparison.

As an example illustrating the evidence to be derived from direct visual comparison reference may be made to FIG. 1, in which are shown in outline the upper dental arcades of man, anthropoid apes, and the Australopithecinae. Now, in the first instance, a direct comparison of man and apes makes it clear that (apart from the possibility of aberrant exceptions) they show certain outstanding contrasts. Thus in man the arcade has an evenly rounded contour, the canines and incisors are relatively small, the canine is placed medially to the axis of the post-canine teeth so as to run in series with the line of the

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[4] S. Zuckerman. *Nature*, April 22, 1950, p. 652.

incisor teeth, and there is no conspicuous gap (diastema) between the canine and the lateral incisor. In the anthropoid apes, on the other hand, the canines and post-canine teeth form more or less straight, parallel rows, the canines and incisors are relatively large, the central incisors lie considerably in advance of the level of the canines, and there is almost invariably a well-marked diastema. The total morphological pattern presented by all these features in combination appears to distinguish man from apes rather strongly. If attention is now turned to the upper dental arcade in six different specimens of the Australopithecinae which are available (FIG. 1, A–F), it is immediately apparent that in all these specimens the arcade quite consistently resembles the hominid rather than the anthropoid ape pattern, as is also the case with a number of morphological details of the teeth (*vide supra*). The question then arises—is there in fact any specimen of an ape known (even if it is quite exceptional) in which the hominid pattern is reproduced to an equivalent degree ? So far as I am aware, this is not the case, and those who have been unable to recognize that in their dentition the Australopithecinae show a closer approach to hominids than do the anthropoid apes can hardly continue to maintain this view without producing such a specimen (5).

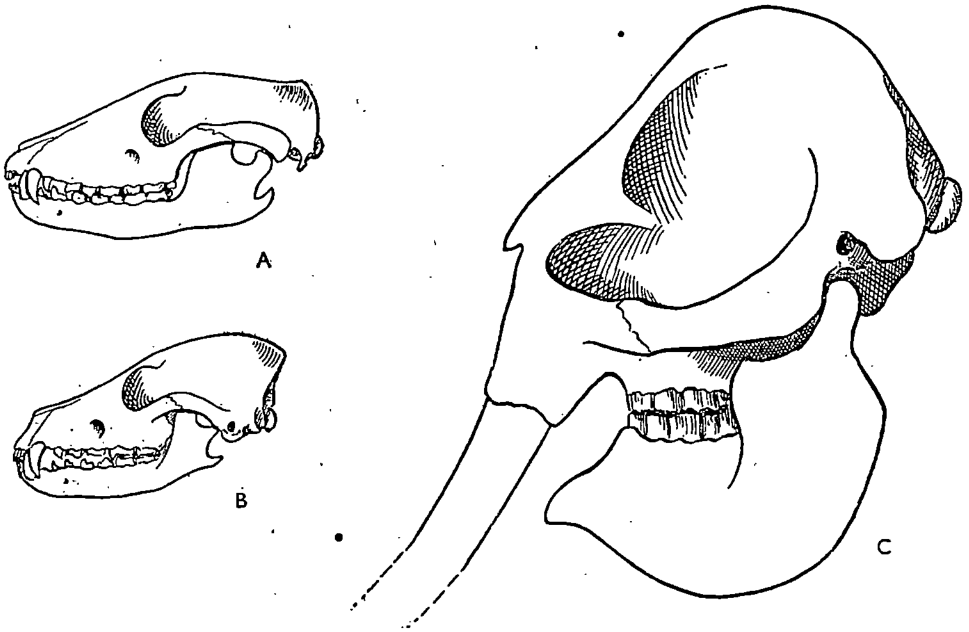

FIG. 2. A DIAGRAM ILLUSTRATING THE SKULLS OF (A) the TASMANIAN WOLF, (B) *CANIS FAMILIARIS* AND (C) THE ELEPHANT

This somewhat extreme example serves to stress the point that a statistical study confined to main dimensions and indices may (if applied without reference to the *total* morphological pattern) lead to very fallacious results in attempting by this method to assess taxonomic affinities

The example of the pattern of the upper dental arcade is but one of many which might be adduced in support of the evidence to be obtained by direct visual comparison.

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[5] Indeed, as already emphasized, it would be necessary to produce several specimens, taken at random from a common group, in order to parallel the consistency with which the hominid pattern is found in the many specimens now available of the Australopithecinae.

This method, it may be noted, is no less accurate than the commonly employed method of statistical analysis of isolated metrical abstractions ; it has, in addition, obvious advantages, one of which is that it can take into account many factors simultaneously. Lastly it is perhaps hardly necessary to point out that, before attempting an assessment of the affinities of the Australopithecinae, a great variety of morphological patterns need to be taken into consideration—not only the pattern of the dental arcades, but also those presented by the morphological details of the teeth, of the skull, and of the limb skeleton.

It has seemed desirable to call attention to the fallacies which may result from an uncritical application of statistical methods to too limited material in taxonomic problems, for, unless these fallacies are recognized, they may give rise to serious confusion in discussions on the significance of the Australopithecinae. That the usual statistical methods do need to be employed with extreme care and circumspection in the assessment of affinities if they are to give relevant answers may be further emphasized by reference to a somewhat extreme (but entirely apposite) example. If attention were limited to major dimensions and indices of the skulls of a Tasmanian wolf, one of the Canidae, and an elephant (see FIG. 2), the investigator might readily find a hundred or more ' characters ' common only to the first two, with the implication that they are much more closely related to each other than either is to the third. But a consideration of the *total* pattern presented by the morphological details of the animal (combined with some appreciation of the real nature of morphological evidence) would be sufficient to demonstrate (what is in fact the case) that the Tasmanian wolf belongs to an entirely separate sub-class of the Mammalia (6) ! A consideration of the total morphological pattern of the dentition makes it equally clear that in many of its features the Australopithecinae approach the hominid rather than anthropoid ape pattern.

Now let us turn our attention to the limb skeleton. Here again, it is clearly essential to consider total patterns rather than isolated characters. Attempts to analyse some of the metrical characters of an isolated specimen of the earlier (very fragmentary) limb bone material, for example, had led Kern and Straus (7) to the startling conclusion that the Australopithecinae might have been quadrupedal primates of a cercopithecoid type—a conclusion which was evidently inconsistent with all the other morphological evidence then available. Fortunately, Dr Broom has again clarified the issue, this time by collecting more remains of the limb skeleton, including the lower end of another femur (reproducing the hominid pattern of characters previously noted in the first specimen), and a practically complete pelvic bone. This last specimen is very remarkable indeed for, in spite of the fact that it possesses certain features in which it appears to be distinguished from *Homo sapiens* of today, in its general build and proportions it is quite clearly much more hominid than ape-like (FIG. 3). It supplies the final argument in confirmation of the South African scientists who already, some years ago, had recognized the hominid traits of the Australopithecinae and had also inferred that their posture must have approximated in some degree to that characteristic of man. It would hardly be possible now to deny this conclusion—unless, indeed, the pelvis is held to be not part of an Australopithecine skeleton at all. But surely this would be a last argument of desperation. For the pelvic bone was excavated actually *in situ* by experienced workers (and it would

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[6] Of course, if the answer were known in advance, it might be possible to select measurements in order to demonstrate statistically that *Canis* really has closer affinities to the Elephant than to the Tasmanian wolf. But this hardly provides a vindication of this method (unless it is applied very critically).

[7] H. M. Kern and W. L. Straus. ' The femur of *Plesianthropus* ', *Am. Journ. Phys. Anthr.*, vol. 7, p. 53, 1949.

be difficult to rival Dr Broom's experience of over 50 years as a palaeontologist), and it was embedded in a dense matrix of stalagmitic limestone in immediate relationship to Australopithecine skulls, jaws, teeth and other elements of the limb skeleton. In their account of the Sterkfontein excavations, Broom and Robinson give a general plan of the site. This makes it clear that all the recently collected material was found within a very circumscribed area of only a few square yards, and also that the pelvis and several skulls lay at practically the same depth in the limestone matrix. It also shows that the pelvis was placed more or less centrally among the other closely packed remains, about 3 feet from one skull and less than 8 feet from another. Lastly, it is important to emphasize that, since the pelvis was found with the femur, crushed remains of vertebrae, etc., it evidently belonged to a skeleton still partly articulated at the time when it was deposited in the position in which it lay. The circumstances in which the pelvic bone was found are thus far better documented than in most fossil discoveries relating to early hominids. And even if there were any doubt on this score, there is now part of a second pelvis (the iliac bone) to provide corroborative evidence, for this specimen, which (it is important to note) reproduces the same distinctive features of the Sterkfontein pelvis, was found at a site, some 200 miles away, which has also yielded Australopithecine remains (but no trace of *Homo*).

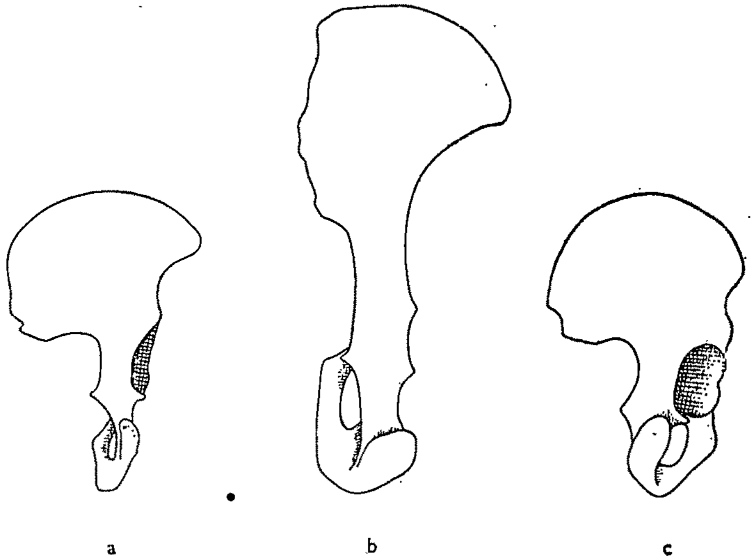

FIG. 3. OUTLINE DRAWINGS OF THE RIGHT PELVIC BONE (VIEWED FROM THE LATERAL ASPECT) OF (a) *PLESIANTHROPUS*, (b) CHIMPANZEE AND (c) *HOMO SAPIENS* (*after* Dr Broom)

With all the remains of the Australopithecinae which have now been discovered by Dr Broom, the real significance of these fossil primates becomes clearer. There can be no question, of course, but that they belong to the same general group which includes the Hominidae and the anthropoid apes—a group now termed the Hominoidea. There can also be no further question but that, in many respects, they show a closer morphological resemblance to the Hominidae than do any of the known anthropoid apes. The obvious implication would seem to be that they are more closely related to the Hominidae and, indeed, it seems a reasonable inference that they actually represent an early phase in the hominid (rather than the anthropoid ape) sequence of evolution (8). This is not to say, of course, that they are necessarily themselves ancestral to *Homo sapiens*—but at least they seem likely to be collateral descendants (not greatly modified) of this ancestral stock. Another possible interpretation, however, calls for consideration—that they are representatives of the anthropoid ape sequence which have developed an amazing parallelism to the hominid sequence (but not *directly* related to the latter). In so far as it is not possible to define the ultimate theoretical limits of parallelism in evolution, such an interpretation can perhaps not be finally excluded, but, were it true, it would surely be a very extreme example of this phenomenon. It must also be emphasized that, morphologically, there appears to be no specialized or aberrant feature of the Australopithecinae which would positively exclude them from some place in the hominid sequence. Indeed, if an attempt is made to construct a hypothetical picture of the ancestral forms which must presumably have immediately preceded the *Pithecanthropus* phase of human evolution, it is difficult to know how such ancestral forms would have differed from the Australopithecinae (except in relatively minor details).

The fact is that, if the Australopithecinae are accepted simply as an unusual example of evolutionary parallelism, the palaeontologist who is searching for a morphological precursor of *Pithecanthropus* finds himself in a very curious position indeed. For he will be driven to search in some other part of the world for fossil remains which, even if found, would not show any really fundamental differences from the South African Australopithecinae which have already been discovered by Dr Broom. And then, of course, he would still have to face the captious criticism that his new discoveries are also nothing more than extreme examples of parallelism !

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[8] The arguments in favour of this interpretation have been presented in some detail elsewhere (*Quart. J. Geol. Soc., London*, vol. 105, p. 225, 1950).

George Grant MacCurdy

by Earnest A. Hooton

Harvard University

THIS appreciation of George Grant MacCurdy does not purport to record either the main facts or the details of his long and fruitful life. It is merely the appraisal of the man, the teacher, and the scientist by one who was his junior friend and colleague for more than a third of a century. It is designed to supplement the admirable biographical sketch written by Dr Hugh Hencken for the *Bulletin* §16 *of the American School of Prehistoric Research.*

Thirty-five years ago, Dr MacCurdy and his friend, Dr Charles Peabody, then Curator of European Archaeology in the Peabody Museum of Harvard University, were practically the only American scholars conversant with the field of European Prehistory and known and recognized by workers in that field. Dr MacCurdy, who kept himself completely abreast of scientific contributions to the subject, served as the principal medium of disseminating this knowledge to American anthropologists, by numerous clear and succinct accounts and reviews of the literature, mostly published in the *American Anthropologist.* He had himself produced the outstanding monograph in English on the subject of Eoliths, which is still worthy of perusal. The idea of the American School of Prehistoric Research was originated jointly by MacCurdy and Peabody, but, after the first season or two, Dr MacCurdy carried the whole burden and responsibility of the project. The School received at this time very little support or encouragement from the American Anthropological Association or from professional anthropological workers in this country. Nels Nelson of the American Museum of Natural History and the writer of this account, together with the late Dr Hrdlička of the U.S. National Museum, were almost the only American anthropologists to manifest an active interest. Nevertheless, Dr MacCurdy with the indispensable and incalculably valuable assistance of his dynamic wife, persisted in the difficult undertaking. American students of Prehistory and of Physical Anthropology who looked beyond the New World horizon, began to flock to the summer field and laboratory session of the School, where they received from Dr MacCurdy and from the many European authorities who were his eager collaborators basic instruction in the field and a first-hand acquaintance with the great collections in European Museums. They also had the opportunity to participate in excavations in many countries of Europe. The results of this MacCurdy effort was the awakening of professional interest and the establishment of courses in European Prehistory in many institutions of this country by the former students of the School.

At the same time, Dr MacCurdy recognized the importance of acquainting educated and intelligent laymen with the fascinating discoveries in the Old World of prehistoric art and culture. One of the first and most important converts of Dr MacCurdy to an enthusiasm for prehistory was the late Addison L. Greene, of Holyoke, Mass., for many years Chairman of the Board of Trustees of the School, and an indefatigable and successful promoter of its interests. Another important addition to the field of amateur prehistorians was General Charles G. Dawes. It was Dr MacCurdy's practice to take with him on the summer sessions of the School a few of these influential and cultured Americans and to indoctrinate them with his own zeal for prehistoric studies by actually taking them to the sites and showing them the ancient works of art and industry. In this effort Dr MacCurdy was pre-eminently successful, largely because of his ability to make things clear to the uninstructed layman, but even more through his own obviously pure and disinterested scientific zeal. While professional anthropological sceptics stood aside and scoffed, Dr MacCurdy laid a substantial foundation for prehistoric studies among American graduate students, raised up new experts in the field from the ranks of young people who had received no previous anthropological instruction of any kind, made helpful friends for the School among persons of maturity who had already achieved distinction in business, in professions, or in statesmanship. At the same time Dr MacCurdy enormously stimulated the actual field work of European archaeologists by giving them assistance in the way of grants from the School, in return for which they taught his students and allowed them to participate in actual excavations.

In 1925 Dr MacCurdy gave a further impulse to studies of Prehistory by the publication of his great work—Human Origins—which has not been superseded up to this day by any treatise on the subject of comparable authority and scope.

The short summer excavations, the museum visits, and the evangelizing of adult lay interest were soon accompanied by the institution of many important and elaborate excavation projects, carried on by advanced students of the School in Europe and elsewhere. Notable were the many careful excavations carried on in Central and South-eastern Europe by the late Dr Vladimir Fewkes and his assistants, and the joint effort with the British School of Archaeology at Mt. Carmel, Palestine, where, under the leadership of Professor Dorothy Garrod, the tremendous discoveries of Neanderthal Man at Mt. Carmel were made. It is perhaps safe to state that no enterprise during the present century has contributed more to the advancement of prehistoric studies in Europe than have the multifarious activities of the American School of Prehistoric Research, all activated by Dr MacCurdy.

George Grant MacCurdy was one of the most unselfish, kindly and tolerant of men. He was wholly incapable of self-promotion and congenitally non-political in his behaviour. While he was able to achieve marvels in the development of his scientific child, the American School, he neither desired nor sought scientific personal distinction and academic preferment. However, he achieved among all who knew him well a reputation for thorough scholarship and skilled teaching that could be envied by any class-room lecturer on anthropological subjects. Moreover, the character of the man was such that meanness, malice, and uncharitableness were wholly alien to him. He was a great gentleman, revered and beloved by his students.

The American School of Prehistoric Research is Dr MacCurdy's monument and should perpetuate his memory, not necessarily in name or in details of present organization, but in its continuing contributions to our knowledge of ancient man and in its raising up of new generations of prehistorians and of amateurs of the study of man.

Jarmo: A Village of Early Farmers in Iraq

by ROBERT J. BRAIDWOOD
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and LINDA BRAIDWOOD
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THERE is a body of theory, most clearly delineated in recent years by V. Gordon Childe, concerning the importance of the appearance of Food-Production as a basic economic revolution. The domestication of plants and animals assured a stable food supply. Proper village life now came into being, and with it a completely new kind of technology. This latter depends on the fact that time now became available for pursuits other than that of simply collecting food. The theory holds it to be no coincidence that such crafts as architecture, pottery, weaving, and presently metallurgy make their appearance with the establishment of Food-production. These crafts make use of materials constructively, and in some cases actually change the physical or chemical properties of the materials. Such a technology was not characteristic of the preceding Food-gathering stage. The Food-producing revolution and the type of technology which attended it were at least the economic prerequisite for the appearance of *civilization* (in any useful sense of that word).

In spite of the relatively imprecise chronology now available to pre- and protohistoric archaeology it is possible to maintain that Food-production made its first appearance in the Near-middle Eastern area perhaps as early as eight thousand years ago. *Civilization*—characterized among other things by urbanization and the formal political state—also made its appearance first in the Near East at *c.* 3000 B.C. There is at least one other independent appearance of Food-production—and of subsequent *civilization*—somewhat later in the New World. The evidence remains unclear as to whether independent beginnings in Food-production were made in eastern or southeastern Asia.

The actual origins of Food-production in the Near East (as elsewhere) are still unclear from the available archaeological evidence. The earliest *clearly defined* village assemblages in the Near East, which are *now in hand*, would seem to be five: that represented by Sialk I in north central Iran, by Hassuna and Nineveh I in northerly Iraq, by the basal Amouq materials and the 'Neolithic' of Mersin in Syro-Cilicia, by Jericho 9 [1] in inland Palestine, and by Fayum A in Lower Egypt. This assessment is made on the grounds of materials available to us or adequately published; we assume that the relatively comparable and simple technology of these five assemblages indicates their chronological contemporaneity in a rough way. We have consciously not included the Tasian of Upper Egypt in the above picture, as we feel it *may* simply be a typological construct of ' impoverished ' Badarian materials. We believe there may be snatches of other assemblages of a date and technological simplicity comparable to the above mentioned five (for example Bakun B-1) but one cannot get at them as yet.

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[1] And Jericho levels 10–17 as well? The flints, at least, seem consistent within the whole range from Jericho 17 through 9. And what indications are available of architectural plan seem also to be consistent throughout some of these levels including 9.

What clearly defined materials precede the available five earliest village assemblages (Sialk I, Hassuna, Amouq-Mersin, Jericho 9, and Fayum A) ? Looking backwards in time from the stage they represent, we see no *clearly defined* materials lying between them and the Natufian of Palestine. Whether the Natufian assemblage is a terminal Food-gathering one (as we tend to believe) or an incipient agricultural stage (as Childe and others hold) is mainly a matter of semantics. We had already in 1941 postulated a gap in archaeological knowledge between the stage represented by the Natufian and that of the five earliest available villages. To our way of thinking, the supposed continuity between the Natufian and basal Jericho (whether by way of the nebulous ' Tahunian ' or not) is a mirage. We believe Abu Usba may suggest a coastal Palestinian early village assemblage (rather than the continuum of the Natufian the excavator suggests) distinct from Jericho. So far there is not enough of it available to go beyond the point of suggestion. There are a few other scraps of material but we are convinced a gap in knowledge exists between the Natufian and the earliest available villages. We also reason that it would be exactly within the time range covered by this gap in knowledge that the essential elements of the Food-producing revolution came into being.

This reasoning pointed the problem for our first field work for the Oriental Institute following the war. The five earliest available village assemblages (Fayum making somewhat of an exception) lie on the hilly flanks of Breasted's ' Fertile Crescent ' : those in western Asia are all above the present 250 mm. or even the 500 mm. isoyhet. We decided to seek a site rather near the apex of the hilly flanks of the crescent in Iraq. Our application to the Iraqi Directorate General of Antiquities was met with enthusiasm. Dr Naji al-Asil, the Director General, provided us with a list and recommendations for some fifty promising sites of prehistoric villages which his staff had noted on survey. We chose for excavation a site called Matarrah, some 34 kms. south of Kirkuk, which had predominantly Hassunan surface materials and which lay well to the south of the type site. We also asked for a sounding permit on a second site, called Qalat Jarmo, midway between Kirkuk and Suleimaniyah. We could not at long range anticipate the full promise of Jarmo. Messrs Seton Lloyd and Fuad Safar had visited it and made surface collections which were reported to consist only of flaked flint tools including some of microlithic size, and a few bits of ground stone. The site was thus assessed as ' Mesolithic ' and until we were able to test it, we were not certain whether it would prove fully pertinent to our central problem. The entire credit for locating Jarmo and for realizing its potential interest for pre- and protohistoric archaeology belongs to the Iraqi Directorate General of Antiquities.

The site at Matarrah yielded a southern variant of the Hassuna assemblage, which is to be reported on in an early issue of the *Journal of Near Eastern Studies.* It did not fulfil our expectations that it might contain pre-Hassunan materials. As to Jarmo, the combination of a one month sounding-permit and the approach of hot weather forced the campaign at Jarmo to be a brief one. We are fortunate at this writing to be *en route* to Jarmo again for a full season's work there (2).

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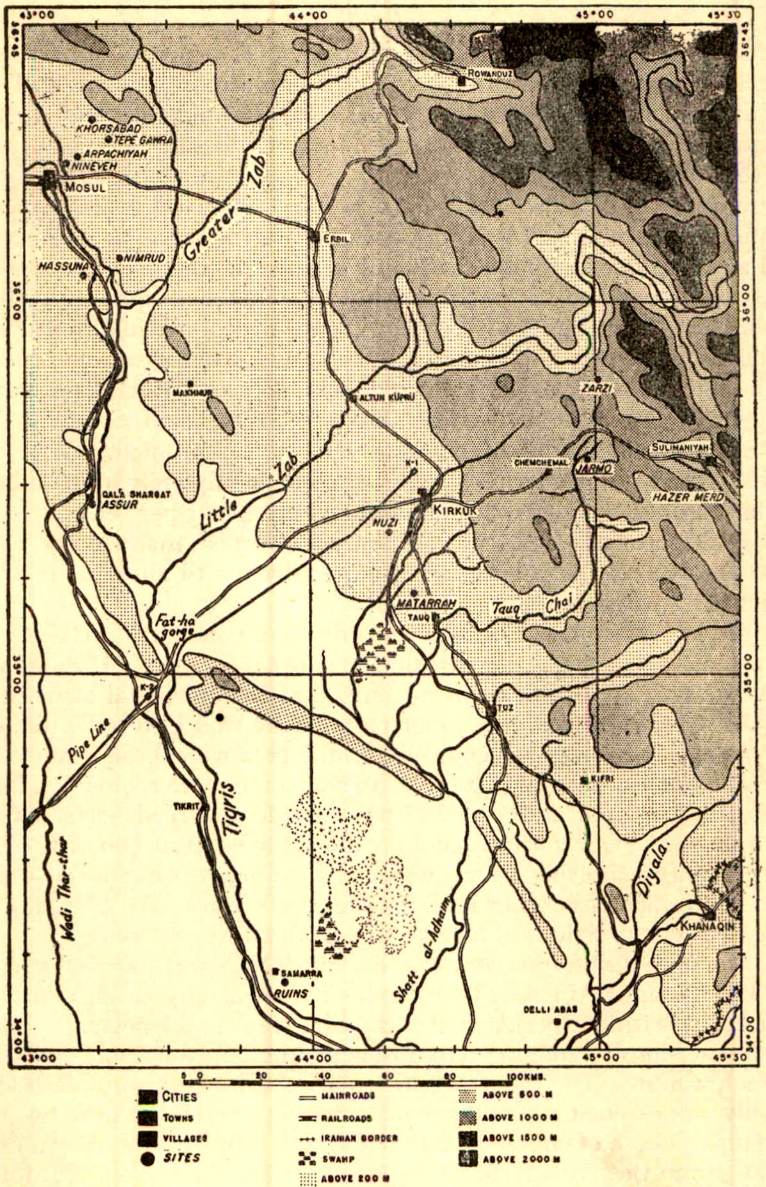
[2] The staff during our 1948 campaign at Matarrah and Jarmo consisted of Dr Faraj Basmachi of the Directorate General of Antiquities, Miss Charlotte Otten, a graduate student on grant from the Department of Anthropology of the University of Chicago, and ourselves for the Oriental Institute. We are deeply indebted to the Director General and all of his staff for their cooperation, and to our colleagues in Anthropology for enabling Miss Otten to be with us.

Jarmo lies on a kind of natural promontory formed by deep *wadi* cuttings. The landscape is one of an upland grassy terrace of top soil on decomposing sandstone, sloping off from the Sagirma ridge to the east, with interruptions by the deep ravines of the *wadis*. The mound debris of the site itself is some 5.0 m. in thickness over the sandstone of the original terrace at the point where we tested it. The debris by surface indication seems to cover an ovid area of *c*. 90.0 by 140.0 m., which makes up most of



the promontory. The west, north, and northeast slopes of the promontory are quite steep and fall *c.* 50.0 m. to the *wadi* bed. There is a perennial spring in the *wadi* several hundred yards northeast of the promontory.

Save for three shallow 2.0 by 2.0 m. testpits, our major effort on Jarmo in the month of May, 1948, was a 7.0 by 10.0 m. area taken down to *c.* 3.5 m. over the whole area, then reduced by half, and cleared to bed-rock at *c.* 5.0 m. depth in only about 8 square metres. Within this depth there were eight more-or-less well-marked floor levels, and hints that several more intermediate levels might develop further into the core of the mound. The assemblage which these levels yielded was essentially consistent throughout the total depth ; the exception to this came in the matter of potsherds.

A meagre quantity of coarse hand-made pottery appeared in the uppermost 75 cm. in our operations at Jarmo. This is, in effect, the depth of the present root line. One or two small and fragmentary patches of cobble-stone paving or wall founding appeared within this depth, and also several fragments of what had apparently been bitumen surfaced ' hearths.' Fragments of laid stone, and of bitumen flooring appeared below the 75 cm. depth, however, and there was absolutely nothing in any other category of artifacts in the uppermost 75 cm. depth which was inconsistent with the materials below, save for the potsherds. The sherds themselves were of small size, with badly eroded edges ; lip sections were available and were very simply profiled. Most of the sherds showed traces of chaff temper ; they appeared to be incompletely fired and their surfaces were somewhat fugitive when we washed them. Perhaps 25 per cent of the bulk showed traces of the addition of a red surface solution, and burnish marks could occasionally be noticed. On the whole, the sherds were too featureless to allow us to compare them with any other early pottery of our acquaintance.

There was very little trace of architecture in the next *c.* 1.5 to 2.0 m. depth of deposit in our main exposure. Incomplete runs of *touf* (i.e., *pisé* or loaded mud) walling with a corner or two appeared, assuring only that their original structures had been rectilinear. At *c.* 3.5 m. depth, the upper parts of the wall butts of a building in level 6 began to appear. This was not complete in the area we exposed, but consisted of at least two fair sized rooms and suggestions of two or three more rooms. There were connecting doors, and the floor of one room bore the traces of reeds—laid lengthwise, as if to form a simple bed. Querns and hand grinders appeared in two of the rooms. At a slightly lower level (presumably a level which will lie between the levels 6 and 7 we first identified) was the trace of another reed floored ' bedroom.' We encountered no architecture in the restricted portions of levels 7 and 8 which we exposed.

At level 5, which had fragments of walls, but which was not well preserved architecturally, we found approximately two-thirds of what had apparently been an elliptical baked-in-place clay basin, of perhaps 50 cms. at its greatest diameter. This appeared to have been built by simply scooping a basin-shaped hole in the floor, facing it with clay, and building a fire in it. The characteristic sherds of these unportable basins (surfaced on one face only) appeared at all depths in our exposure, and there were two other smaller *in situ* fragments. These of course recall the basins Professor Garstang noted in Jericho 9, save that at Jarmo they run through a succession of levels. We did not recover any fragments of these basins which could be called rim-sherds.

There were practically no other utilitarian (to our mind) objects of baked clay in the Jarmo assemblage. Small clay cones and bulla-like blobs of lightly baked or unbaked clay appeared in quantity. There were simple clay beads and a number of figurines. These were predominantly of animals but there were also convincing representations of human heads and of a seated type of mother goddess. Only a small portion of these figurines had been fully fired.

The lack of spindle whorls in either clay or stone might be noted. There were, however, fragmentary impressions of simple over-and-under reed mats, and also of a somewhat more complicated type of mat or basket weave—the ghost of this latter is not completely clear.

In the flaked stone category, the situation at level 6 can probably be taken as typical. Of the total number of flints and obsidian found in level 6 (*c.* 4000 including worked examples and mere chips) about 40 per cent are normal or large size artifacts and roughly 60 per cent are of a microlithic size. Obsidian was used in a fair quantity but in the main only for microliths. The industry is predominantly a blade tool industry. The retouch is neat. The tool kit seems to be very limited. The larger size tools include sickle blades, borers, and scrapers. The microliths are fairly monotonous in that the majority are neat, sharply pointed blades, used without further retouch. The small proportion that has been retouched is mainly borers but also includes an occasional blunted back blade. Geometric microliths, except for five or six examples scattered throughout the levels, are non-existent.

It is pertinent to point out here that Jarmo lies only 15 miles from Professor Garrod's cave at Zarzi. Zarzi seemed to her to be an extended Gravettian industry with a microlithic complement. The Zarzi microliths even though few in quantity show a variety of types not found in the simple Jarmo repertoire. It would seem that despite the great abundance of microliths at Jarmo, their intrinsic monotony, taken in conjunction with the rest of the Jarmo assemblage, suggests a tapering off of interest in tools in the microlithic category.

There is considerable variety in ground stone tools. The presence of querns and hand-rubbers has been noted, and there were also boulder mortars and pestles. Included under the name of ' pestles ' were numbers of perfectly shaped small and even minute pieces—we do not know their utility. There was a flourishing industry in finely ground stone bowls ; as with the small pestle-like pieces, considerable care seems to have been given to the selection of handsome varities of stone, and to the cutting of the bowls so that the coloured veins of marble-like stones would appear as banding on the bowl. Full ground celts were available but in no great number ; both axes and adzes are present. There were large sized pierced oblates of stone which we suppose may have been dibble-weights. Under the non-utilitarian ground stone category came a phallus and a variety of beads mainly simple but including a few more elaborate forms such as a long bead of flattened lentoid section that was collared. There were also many bracelet fragments usually of marble, some with grooved decoration. Some of these also had holes for segmental binding. There were a few animal figurines in stone and also several stone ' pegs ' or ' nails.'

The bone category was rather simple ; it included articular butted or broken butted awls, fragmentary rib-blades, delicate needles, and gorget fragments. There was also a peculiar sort of shaft-hole double gouge-like tool, and a long handled, shallow bowled spoon.

In shell, we noted only a fragmentary double pierced pendant.

Much of the interest of the catalogue of Jarmo materials lies in the non-artifactual materials. Such grain as we did find was badly carbonized ; it was given a preliminary identification as both ' wheat ' and ' barley ' by botanists in the United States Department of Agriculture, and has now gone into the hands of Professor Paul Mangelsdorf of the Department of Botany at Harvard University for further study. The animal bones were given over to Bryan Patterson, Curator of Vertebrate Paleontology of the Chicago Natural History Museum. Dr Patterson's preliminary study has gone so far as to indicate the presence of sheep and/or goat, cattle, pig, dog, and several equid teeth. Specific recognition has so far not been completed but Dr Patterson is of the opinion that *c.* 95 per cent of the bulk of the Jarmo faunal remains fall in the above categories, and that if these beasts were not ' domesticated ' in the full sense of the word, they were at least ' potentially domesticable.' Wild animal forms made up the remaining 5 per cent of the bulk. In all levels at Jarmo but especially concentrated in levels 7 and 8 there were land snails. A number of these were given over to Fritz Haas, Curator of Lower Invertebrates of the Chicago Natural History Museum. A specific recognition is not yet available, but Dr Haas is of the opinion that the ecological situation suggested by these snails would be one of open grass lands (such as now obtains at Jarmo).

There were several flexed sub-floor burials at Jarmo. No grave-goods appeared with them. The skeletons were themselves in too fragmentary a condition for restoration, but the teeth have been given over to Dr Albert Dahlberg, Research Associate of the Department of Anthropology of the University of Chicago for further study.

We were particularly fortunate in having a sufficient quantity of the Jarmo levels 7–8 snail shells for a single ' run ' by the Carbon 14 dating project, under Professor W. F. Libby of the Institute for Nuclear Studies of the University of Chicago. Dr Libby and his colleague, Dr James Arnold, have remained somewhat restless about the reliability of shell, especially when only one ' run ' is available, and when there is no check with a vegetable carbon material. Hence the result can only stand as tentative. It is 6900±320 years ago, or within the range from 5270 B.C. to 4630 B.C. (subject to the theory of probable error as Dr Libby applies it).

Jarmo has by no means closed the gap in archaeological knowledge between the stage represented by the Natufian of Palestine and the five available earliest village assemblages. On typological grounds Jarmo is certainly pre-Hassunan, but post-Natufian. In fact it is probably much closer to the Hassunan than to the Natufian stage, typologically speaking It does not, however, have as many traits which would clearly suggest themselves as antecedents for Hassunan traits as one might have suspected. This generalization may, of course, need revision when a larger exposure is made at Jarmo.

We are quite conscious of the fact that Jarmo is up in the hills, well above the typical Iraqi plain wherein Hassuna, Nineveh, and Matarrah lie. This raises the question of whether it might be a ' back-water ' culturally speaking, and hence not chronologically earlier than the Hassunan assemblage. We do not think this is true, nor did those of our colleagues who visited the site. The question must, however, remain open until both the Jarmo and Hassuna stages can be adequately fixed by Carbon 14 or until an assemblage intermediate to Jarmo and Hassuna is discovered and intercalated between the two.

Jarmo has also stimulated our interest to know more of Jericho 10–17. Save for the excellent treatment of the flints, this range of Jericho has been a depressing one to handle, due to inadequacy of publication. It seems to us quite probable that it represents a stage similar to that of Jarmo. Since 1948 several other sites have been worked, which—when details concerning them become available—may also prove to fit somewhere into the general picture. These are the cave sites worked by Professor Carleton Coon on the Caspian terraces in northern Iran, by M. Roman Ghirshman in the Bakhtiari hills in Iran, and M. Mosé Stekelis on the Yarmuk in Palestine.

A final observation, however subjective, came to us through handling the Jarmo materials at the same time as we were processing those of the Hassunan stage Matarrah site. What struck us was the contrast between the ' feel ' or ' character ' of the various artifacts from Jarmo, as against those of Matarrah. It was a contrast of dynamic with static, so to speak; as if the Jarmo craftsmen had been in a phase of productive intensity and creativity, whereas the Matarrah craftsmen were simply producing—in a rather spiritless way, as if they had reached a peak and then levelled off.

We would like to believe that this observation does suggest something. Even though Jarmo does not by any means close the gap in archaeological knowledge, we suppose it to be the most substantial assemblage now available *nearest* to the beginnings of Food-production. When Food-production first came into being, there must have been a remarkable release of cultural potential, just as there was following the time of James Watt and the beginnings of Industrialization. In this sense, the burst of technological activity which Jarmo seems to indicate may roughly parallel that of our own times (**3**). But as is usual in prehistoric archaeology, a considerably greater bulk of material than is now in hand must be reclaimed and interpreted. When this is done, the rest of the picture—in its sociological, religious, aesthetic, etc., aspects—will become as clear as archaeology can ever make it.

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[3] This does not necessarily mean that Jarmo stands just two centuries following the beginnings of Food-production, as we do from the beginnings of Industrialization. The actual rate of acceleration was probably slower. But before Dr Libby's date became available, we had guessed that Jarmo might have an antiquity of *c.* 6000 B.C. The later Carbon date does not depress us, however. It suggests rather that once Food-production came into being, the rate of cultural acceleration—relative to those times—was as remarkable as the rapid technological advances of our own.

Palestinian Excavations

by KATHLEEN M. KENYON

BETH-SHAN. Vol. II. Four Canaanite Temples. Pt. I. The Temples and Cult Objects. Alan Rowe. Philadelphia: published for the University Museum by the University of Pennsylvania Press. $15. XII+101 pp., LXXI plates. 1940.

MEGIDDO II. Seasons of 1935–9. Gordon Loud. Published by the University of Chicago Press as the University of Chicago Oriental Institute Publications, Vol. LXII. I vol. of text, XXI+199 pp., 416 text figures, I vol. plates, 91 plates line blocks, 199 plates photographs, with accompanying description. 1948.

EXCAVATIONS AT TELL EN-NASBEH. Published by the Palestine Institute of Pacific School of Religion and the American Schools of Oriental Research. Vol. I, Archaeological and Historical Results, by C. C. McCown. XXII+322 pp., 112 plates. Vol. II, The Pottery, by I. C. Wampler. 186 pp., 90 plates. 1947.

RECENT years have seen the publication of some sumptuous reports of the large-scale excavations conducted by American enterprise in the years before the war. The sites of Megiddo and Beth-Shan, which can confidently be referred to by their historical and biblical names, are of outstanding importance, dominating as they do the Plain of Esdraelon and the great road from Egypt to North Syria and Mesopotamia. The publications here considered are the latest (but not, it is hoped, the last) of a series dealing with different aspects of the excavations. Both sites have been partially sounded to bed-rock, and show continuous occupation from the chalcolithic period to the end of the first millennium B.C., and Beth-Shan beyond it. Tell en-Nasbeh is in a different category. It is possibly to be identified with the Biblical Mizpah, but this is not universally accepted. Like many Palestinian hill-country sites, it was occupied in the Early Bronze Age (the ascription of some groups to the chalcolithic period is unsatisfactory). Its main occupation is, however, confined to the Early Iron Age, from the time of the undivided Israelite Kingdom down to the post-exilic, Hellenistic and Roman periods.

The Megiddo excavations are the better and more completely published and probably the better executed. The present volume is concerned mainly with strata XX (the lowest) to VI, with some additional matter on strata V and IV, already fully published in *Megiddo I*. The pottery of strata XX to VI has already been summarily but most usefully published (1). It is here re-published with full descriptions and adequate scale drawings, and it is satisfactory that an interim publication has not tempted those responsible to scamp the full publication, a failing which besets so many excavators.

The foreword emphasizes the fact that neither the excavations nor the publication are as complete as had originally been intended. The original plan had been to excavate every stratum from top to bottom of the Tell completely, an undertaking which must have taxed the resources even of the Oriental Institute in its prime (the area of the summit is 13 acres, the base being considerably larger, and the maximum depth of deposit some 20 metres). Such a scheme bears tribute to the breadth of vision of the late J. H. Breasted, but it may be doubted whether it is a wise one. In England at the present time emphasis

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[1] Shipton, 'Notes on the Megiddo Pottery of Strata VI–XX'. No. 17 of *Studies in Ancient Oriental Civilization*, published by the Oriental Institute of the University of Chicago.

is being laid on the necessity of complete horizontal excavation on selected sites. But such sites should be of manageable size and preferably of restricted period. It is very questionable whether key sites, giving important successions of culture, should be completely excavated. Excavation technique is, and should be, always progressing, and sections of the site should be left for the findings of the earlier excavations to be checked and amplified by later methods. The original Megiddo scheme was, however, abandoned when stratum V was reached, owing to the constriction of the resources of the Oriental Institute in the economic depression of the thirties, and even the more modest scheme to excavate to bed-rock in certain sectors only was not completed in various planned linkings-up, owing to the war.

The war was also responsible for the publication of *Megiddo II* being much less exhaustive than that of *Megiddo I.* The authors only claim that it is ' a catalogue of the architecture and artifacts recovered primarily in the seasons of 1935–39 '. No attempt is made to give comparative material nor to discuss implications. It was felt that the steady withdrawal of members of the staff for war-work demanded a choice between this type of publication immediately, or indefinite delay. The choice was probably rightly made. Users of the volume have to make their own comparisons and conclusions, but at least the material is fully available.

In a review, only a few of the most salient points of importance can be noted. Perhaps the most outstanding is the contribution knowledge of culture and of chronology in Palestine in the fourth and third millennia B.C., which comes from one cutting to bed-rock (BB) and a separately published (2) clearance (for the purpose of dumping) on the slopes, called Megiddo Stages VII to I. The earliest occupation (–XX) was found to be characterized by a flint industry of the Canaanean type, which was shown at Jericho to differ from the Neolithic type of Jericho IX, and to appear in the Chalcolithic (I prefer Garstang's original nomenclature) Jericho VIII, continuing throughout the Early Bronze Age. In Megiddo XX, the same flint industry (with only slight differences) is accompanied by pottery, of a type linked to (though not identical with) Ghassul IV, Jericho VIII, and Beth-Shan XVIII. The particular interest of this lies in the fact of the appearance of the manufacture of pottery at two different stages in Palestine. At Jericho it appears in IX, a level characterized by a neolithic Tahunian flint industry, and, as at Megiddo, going back to before the appearance of pottery. There is probably a gap at Jericho between IX and VIII, representing most of the occupation at Ghassul, and the above mentioned parallelism between Megiddo, Ghassul and Jericho makes it clear that the first pottery level at Megiddo is very considerably later, and that it belongs to a cultural phase different from this event at Jericho. The evidence therefore suggests, though it needs confirmation, that pot-making was an indigenous product of Jericho (for which there is other suggestive evidence), and was not generally introduced in Palestine (for Megiddo is unlikely to be a backward site) until considerably later.

From Megiddo XX onwards, there is an unbroken succession right down to historic times. The evidence from Megiddo and Jericho in combination provides useful pegs for joining up Palestine with Egypt and Mesopotamia, and therefore with fixed chronology. The details of the evidence cannot be given here, but may be summarized as follows: Early Bronze Ia and Ib equals late pre-Dynastic period in Egypt and Mesopotamia; E.B. II equals Egyptian Old Empire; E.B. III equals Pyramid Age of Egypt; the degenerate E.B. IV and the succeeding period, called by Albright Middle Bronze I, but more satisfactorily called Intermediate E.B.–M.B., since it is an intrusive

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[2] Ensberg and Shipton, ' Notes on the Early Pottery of Megiddo '.

culture quite unrelated to the subsequent Middle Bronze culture, equals Egyptian 1st Intermediate.

The importance of the Megiddo publication, therefore, needs no emphasizing. The publication is lavish, and the documentation for the most part adequate. The lack of evidence about stratification is the weakest point of the publication. The only published sections are of the primitive type with walls floating in air, occasionally joined by dotted lines, with no record of the *actual* strata. The reader has therefore no data for assessing the correctness of the attributions to the different strata.

The publication of the Four Canaanite Temples of Beth-Shan was preceded by the publication of the pottery from these levels (3) which must here be considered with it. The four temples are assigned to the periods of Amenophis III, Seti I and two to that of Rameses III. This is on the grounds of the discovery of inscriptions or scarabs of these rulers in the associated levels. Weight must of course be given to such finds, certainly as *termini post quos* and as absolute dating if they are certainly in position. But in the instances under consideration this is not the case, the stelae not being *in situ* and the scarabs, though some were in foundation deposits, may, as Albright suggests, have been out-of-date *ex votos*. There is therefore clearly a risk that stelae may be purposely transferred from one level to another, and of small objects such as scarabs being in fact, in debris, or alternatively being treasured as valuables over a considerable period. This is actually shown by the fact that between the publication of *Beth-Shan* II.II and *Beth-Shan* II.I, the level previously ascribed to Rameses II on the basis of finds of scarabs, has been brought down to Rameses III in view of the discovery of an inscription. Therefore it is necessary to consider the evidence, confirmatory or otherwise, of the pottery and other datable finds. A survey of this evidence leaves no doubt that the periods assigned must be very considerably modified.

This is not the place to discuss in detail the comparisons which suggest a revision of dates; the need for them is generally admitted, and it is hoped to publish such a discussion soon elsewhere. It must be admitted that much additional evidence has appeared since the publication of the pottery report in 1930 and some of it since the report on the structures appeared in 1940. On this evidence, from Megiddo, Tell Abu Hawam, Tell Beit Mirsim and Samaria, it would appear that the 'pre-Amenophis III, 1447–1412 B.C.' level should be dated to 1350–1300 B.C.; the 'Amenophis III to Horemheb, 1411–1314 B.C.' temple to 1300–1150 B.C.; the 'Early Seti I' level to 1150–1100 B.C.; the 'Late Seti I' level to 1150–1000 B.C.; and the 'Rameses III' temples to 1000–850 B.C.

The publication of *Beth-Shan* II.I is thorough. The architectural details are fully discussed, and it is interesting to see the extent of Egyptian influence in earlier temples, revealed also by many of the objects found. The religion practised was clearly the syncretistic affair general in Palestine in the Bronze Age, but the worship of Ashtoreth, with strong chthonic tendencies, played the dominant part, perhaps in association with that of a local warrior god Mikal, later assimilated to Resheph and superseded by the Philistine god Dagon. The cult objects were numerous, and are well published and exhaustively discussed. But as at Megiddo, the stratigraphy is inadequate. The section through the Tell (Pl. I) is entirely schematic, and those through the Temples, though it is refreshing to find them, inadequate and unrealistic.

It is, however, in the excavation of Tell en-Nasbeh that the most deplorable absence of stratigraphical digging is evident. One must sympathize deeply with anyone who has to prepare a publication after the death of the excavator. The authors have made in

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[3] *The Four Canaanite Temples of Beth-Shan.* Pt. II. 'The Pottery'. G. M. Fitzgerald. Philadelphia, 1930.

many ways a magnificent job of it, but one's sympathy is tempered by observing that they do not realize the true inadequacy of the material upon which they have to work. Numerous statements are made throughout the volumes such as ' there was no clear stratification during a good part of the period covered because there was no complete destruction and rebuilding at any one time ' (Vol. 1, p. 10).

It must be confessed that the excavation of Tell en-Nasbeh is an outstanding example of a belief which should have died out in the nineteenth century, that a Professor of Classics, with no field experience, is capable of excavating a Roman site. It was considered that a Professor of Old Testament Literature and Semitic Language was, as such, competent to direct a large scale Palestinian dig. One would not wish to be unkind to the much-respected memory of Dr Badé, but the matter is of such importance to the future of Palestinian Archaeology that the criticism must be made. It is still more important from the fact that Dr Badé did indeed pay much attention to excavation technique, and even as the result of his Tell en-Nasbeh experience produced a Manual of Excavation. He was fully alive to the importance of the exact and complete recording of finds. But this, without an understanding of stratigraphy in the modern sense, is worthless. The result is that everything in the Tell dig is recorded by *locus*, which mostly means a room of the first-reached structure. All the baskets were numbered, counted, sorted and indexed, but digging apparently proceeded from the top to the bottom of the *locus* without reference to the relation of the layers of the soil to the structure. It need not therefore be said that in fact there is no real evidence for dating any structure, only a balance of probabilities. Pl. 3.2 is an almost perfect example of how not to excavate—an untidy trench along the face of the city wall, cutting all stratification running up to it.

The publication is extremely thorough; 59 pages are devoted to the discussion of the identification of the site with the Biblical Mizpah. The only result is to show that there is nothing conclusively against it; one school of philologists says emphatically that it is impossible for the name Mizpah to develop into Nasbeh, while another says that it is quite possible.

The discussion of the buildings is almost entirely vitiated by the lack of chronological evidence about them. The plan does show an apparently interesting town lay-out. But ' it is often impossible to determine whether, on the plans, walls of more than one period may not have been combined [so as] to give an illusory appearance of crowding ' (Vol. 1, p. 228). Therefore, it is useless to spend time discussing it, though reference must be made to the interesting so-called ' four-roomed building ' (p. 206 ff) in which it is clear that in at least two cases the supposed three parallel rooms (with the fourth across the end) are in fact a single, aisled, room, with piers (surviving completely in one case and partially in another) resting on sleeper walls. A house basement 1.10 m. high, again, is surely inadequate as a ' shelter for asses, sheep, and goats ' (Vol. 1, p. 213). But there is enough to show that a great opportunity has been lost on a site entirely suitable (see p. 197 above) for complete excavation.

The publication of the pottery and other finds is very full, but again much of it loses usefulness from the lack of observed stratification. The reproduction of the photographs by collotype is not a satisfactory medium. There are, however, tomb and cistern groups which are valuable, and their date is very fully discussed. The excavator of a Near Eastern site has always to make a difficult choice between the publication of a type series and that of groups as such, with inevitable repetition. Probably the type series is the better solution (if both cannot be done), but it must be thoroughly cross-referenced, which is not done here, for there is nowhere discoverable a full list of the types occurring in any one tomb.

An immense amount of labour has been expended on the discussion of the date of the tombs, but the result is unnecessarily confusing. An elaborate statistical method has been adopted. All parallels in form to a pot-type have been traced, including M.B. ones to a clearly E.1 pot (differences in technique being ignored). The result, as a graph of frequency, is plotted for the tomb, in combination of the frequency graphs of other forms. This gives a series of peaks which more or less coincide, but gives the ludicrous result that the extremes of the graph stretch from the seventeenth to the second centuries B.C. On the other hand, no account is taken of the appearance or non-appearance of diagnostic forms or techniques, on the basis of which one need have little hesitation in giving terminal dates of *c.* 900 B.C. to Tomb 54, *c.* 850 B.C. to Tomb 32 (the identification of the scarabs as 25th Dynasty is doubtful, and they must here belong to an earlier, similar, group of which the existence has already been suspected) and probably to Tomb 5, and *c.* 800 B.C. to Tomb 29.

These three publications have made it possible to review the state of our knowledge of the archaeological history of Palestine, and the weaknesses in past methods. The pause in excavation there, caused by the war and the later troubles, has provided leisure for bringing publication up to date and for considering future aims and methods. It is to be hoped that the time is not very distant when active work on still obscure sites and problems can be resumed. It is also to be hoped that it will then be directed by excavators who understand the principles of stratification and can apply them.

Correspondence

Dear Sir,

At the end of the review of *Cambridge Excavations in Minorca* in your September number, it is stated that the cost of the three expeditions was borne by this Museum thanks to the very generous contributions of its Director. I want to make it clear that the Director referred to was Mr Louis Clarke who was Curator at that time.

Yours faithfully,
G. H. S. BUSHNELL,
Curator, University Museum of
Archaeology and Ethnology, Cambridge.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

PROCEEDINGS OF THE PREHISTORIC SOCIETY, New Series, Vol. XV (1950): 204 pages. London agents: H. K. Lewis & Co., 136 Gower St., W.C. 1. [The articles cover a wide range: the Sudan stone age, British mesolithic to Iron Age, neolithic house-types in temperate Europe. Every article is suitable for inclusion in this section; Dr Clark's first report on the mesolithic site he is digging at Star Carr is of special importance].

SHAFTESBURY, DORSET: THE STREETS, ROADS AND LANES, by E. JERVOISE. [The first publication of a new society; a praiseworthy first attempt to restore its medieval plan].

ARCHAEOLOGY AND THE AFTER-LIFE IN PAGAN AND CHRISTIAN IMAGERY, by I. A. RICHMOND. 57 pages, 9 plates. Cumberlege, 1950.

NEW STUDIES ON RHODESIAN MAN, by J. DESMOND CLARK and others. *Journ. R. Anthr. Inst.*, LXXVII, 1–32. [A re-examination of the Broken Hill discoveries].

ARCHAEOLOGIA GEOGRAPHICA: published by the *Hamburgisches Museum für Völkerkunde und Vorgeschichte*, Binderstrasse 14, Hamburg 13.

APERÇU de la structure agraire du Pays de Galles [Wales] occidentale: la structure agraire du Devon et du Cornwall et les enclosures des XIII et XIV siècles: par PIERRE FLATRÈS. *Annales de Bretagne*, LVI (1), 1949. Rennes.

LES EMAUX CAROLINGIENS DE LA CHASSE DE ST. MARC À HUY SUR MEUSE, ed. MARION, 1948. 450 *fr. belg.*

CARTOGRAPHIE PRÉHISTORIQUE, par L. R. NOUGIER. *Bull. Soc. Préh. de France*, XLVII, 1950, 154.

GEOFFREY OF MONMOUTH AND THE LATIN CHRONICLERS (1300–1500), by LAURA KEELER. *Univ. of California Publications*, Vol. 17.

DIE ADLERFIBEL VON 1936, und andere Falschungen aus einer MÜNCHENER GOLDSCHMIEDEWERKSTATT. *Germania*, XXVIII, 1944–50, 54 ff. [Describes fakes of costly Merovingian jewellery made for Nazis and museums under Nazi rule].

SOME ENGLISH PARALLELS TO THE ANGLO-SAXON POTTERY OF HOLLAND AND BELGIUM IN THE MIGRATION PERIOD, by J. N. L MYRES. *L'Antiquité Classique* (Brussels) XVII, 1948, 453–72.

Notes and News

HOW TO USE OLD MAPS

The following notes are written down at the suggestion of a colleague to whom I had been telling my recent experiences. To my objection that fault-finding was rather thankless business and apt to lead to controversy he replied that criticism was good for people and that it didn't matter what they felt or did about it. I was inclined to agree.

Many old maps of all kinds, from cadastral maps of a parish to world maps, have been reproduced in facsimile by a photographic process, and there is generally an accompanying text. The facsimiles are nearly always so much reduced that the names are illegible. It would be easy to print a list of the names, arranged in some order that can be followed on the facsimile, but this is very seldom done. What purpose, then, does the facsimile fulfil ? Beyond showing the extent of a coast line (if any) known at the time—none. The student wants to know the NAMES. Such a list would be far more valuable than the descriptive matter of the text usually is. A better course would be to reproduce a clearly legible facsimile—if necessary in sections. The only use of a facsimile is to save one the trouble of going to see the original (which is often impossible, especially nowadays). Some of the facsimiles in De la Roncière's ' Découverte de l'Afrique au Moyen Age ' are wholly illegible, and the originals in Italy. Mannoni's facsimile of Egyptus Novelo is good and legible except where the names are concealed by the binding. It would have taken only a few minutes to copy these from the original (on which they can quite easily be read) and print them in the text. This was not done, and I had to go to Paris to do it myself. Such imperfect publication is unscholarly, and if not useless at any rate falls short of completeness. Ten minutes work on the original would have made all the difference.

The same criticisms apply to the scale when the original map has one. The figures are often illegible on the facsimile, nor is the unit always certain. A simple statement in the text would remedy this.

Students also want to know where they can consult the best detailed account of the map. Such accounts are often open to all the above criticisms, but they may be a little better than those given in books of a general character, such as histories of geographical discovery. Such bibliographical information must be known by the author and should be given.

Students who cannot read facsimiles have to travel to see the originals. They should be told where these are, and not only in what town but in what institution in that town. One may not be able to go there, but one can at least write and ask for a photostat, and ' Rome ' and ' Venice ' are insufficient addresses. If one goes, one has to write first to be sure that the institution will be open or the map still there. It is also unscholarly to give the name of the institution without the town in which it is ; it may be considered to be of world-wide repute but one may not happen to have heard of it.

In attempting to identify the places named on an old map it is often assumed that its author was careless or inaccurate. He may have been, but he probably made the best use of the sources at his disposal, such as itineraries of actual journeys or voyages. The order in which these appear, on a coast-line or along a river, may be correct, even when the actual distances apart and positions are wrong. If a place X, which is unknown, is put between two known places, A and B, it is to be looked for, and may often be found, between these places on the modern map. Even if not, it is more likely to be there or thereabouts than 300 miles away on the other side of a range of mountains, where there is another place whose name begins with the same letter, or has some superficial resemblance to it.

In old maps there is a tendency for the same name, sometimes with variable spellings, to recur on a long sequence of maps. A name that cannot be identified on one may, either by the last mentioned method or otherwise, be identifiable on another. Sometimes the latest spelling provides a link with the modern form.

The course of roads may often be determined with precision by noting the places which lie (1) on it (2) on each side of it. By using this method it was possible to plot on the modern map the course of Ogilby's roads (1672) which, being strips or traverses, do not show much of the country through which they pass. But he rarely if ever placed a village on the wrong side of the road.

Places may often be identified by their *relative* distances apart, even when their *absolute* distances (as shown by the scale) are wrong. A place X may be twice as far from A as from B and be found to agree with the position of a known place. It is usually better to try several distances between X and known places before deciding where to place it, and to use proportionate distances, not actual ones.

In setting out to study an old map the first thing to do is of course to locate the places named on a modern map. It is best to begin by making a copy of the original (or facsimile), eliminating any irrelevant matter and drawing it with north at the top. For expository purposes the published account should consist of (1) a legible facsimile (if one does not already exist) (2) a copy of the original drawn as above (3) an outline taken from the modern map with the names on the old map shown (when possible) in their correct geographical positions. It may often not be possible to reproduce a facsimile, though this should always be done when one sets out to produce the definitive account of a particular map rather than the general study of a region. The text should give the reasons for the identifications, and (in the latter case) a list of all names on the original.

The proper elucidation of an old map is a fascinating task, but it involves a lot of hard work, and the drawing of diagrams. It should be remembered that the names have rarely if ever been *invented*; they may be strange and unrecognisable, but they are probably based ultimately on a real place-name, and it is the business of the student to trace its history and identify it. Without such an attempt the text is apt to degenerate into mere useless verbiage, describing what can (with good luck) be seen on the facsimile. Such texts resemble the old-fashioned museum labels which tell one what one can see for oneself and the name of the donor, but omit the provenance. There are many such texts, and it is in the hope of reducing their number in the future that I have written this note.
O.G.S.C.

MAPS OF THE A-E. SUDAN

In our June Editorial Notes we criticized the Sudan Government maps and suggested that 'air survey might have had priority over some other forms of expenditure' We have received a letter from the Sudan Government Survey Department, written to 'refute some of this implied criticism'. The writer admits that the basic topographical map of the country (1 : 250,000) is often inaccurate and out of date, especially in desert areas, but points out that the land alongside the Nile as far south as Lat. 13°N. is covered by cadastral plans on scales ranging from 1 : 2500 to 1 : 4000. The demand is small, but is met by sun-prints. The main purpose served is administrative, and though

' they do not always include much topographical detail, they do show any islands which are permanent and large enough to have been settled'. He goes on to say:—

' In addition to these cadastral maps maintained by us, the Egyptian Government has started to publish a series of 1 : 5000 contoured maps in the areas of the 2nd and 4th cataracts, which they have surveyed in connection with the proposed reservoir. Only a limited number of copies of the maps are at present available but an extra set was secured specially for the Commissioner for Archaeology.

' In addition to these large scale maps we have taken some trouble to secure, through the kindness of the United States Government, copies of some 60,000 air photographs which were taken over the Sudan by U.S.A.A.F. during the recent war. These air photographs can be consulted here and copies of individual prints can also be supplied on payment. Both the present and the late Commissioners for Archaeology have made frequent use of these photographs. We have ourselves already mapped from these photographs about 40,000 square miles ; but it will naturally take a long time to map completely the half million or so square miles of country which they cover. We have also just recently obtained an Eagle IX air survey camera and hope to begin using it this winter in one of the Dove aircraft maintained by the Government air service. This camera was not obtained only for survey work and it will also be available for use by other departments, provided they can pay for chartering the aircraft. Most of these air survey developments were described in a recent article in *Sudan Notes and Records* (Vol. XXX, Part 1).

' I hope that these remarks will remove from your readers' minds the impression which your note must have created. The Sudan is as much larger than England as its resources are smaller, and one cannot expect accurate maps to cover it completely. Nevertheless, in areas that justify it economically, the position is far from being as bad as your note suggests'.

ROMAN LIBYA

The following report is reprinted from *The Times* of 28 September, 1950, by kind permission of the Editor.

A British expedition has just completed a two-month programme of archaeological reconnaissance and survey in the Syrtic and Cyrenaican regions of Libya, with the object of collecting information for a proposed map of Roman Libya. The expedition assembled in Tripoli early in July, and moved eastward along the coast of the Greater Syrtis to Benghazi, arriving there after having visited and surveyed many ancient sites on the route.

During August reconnaissances were carried out in the Gebel area of Cyrenaica and in the pre-desert zone to the south, with the assistance of the British military authorities in the territory. A two-week exercise ' Roman Swan ', organized by Headquarters, Cyrenaica District, enabled the archaeologists to visit many sites not easily accessible. Air survey and photography were made possible by the collaboration of the Royal Air Force.

The British Administration of Tripolitania and the Government of Cyrenaica both gave the greatest encouragement and help to the expedition, and the observations made should assist the compilation of an inventory of those major ancient monuments for the preservation of which the future Libyan State will be responsible.

The areas visited include the region of Sirte (Tripolitania), rich in concrete-built farmhouses of the Roman period, the Mersa Brega zone, with the ancient city of Boreium, and the forts which defended southwestern Cyrenaica from the tribes of the Syrtica, the Barce and Beda areas of the Cyrenaican Gebel, and the desert route between El Abiar and Mechili. Many ancient buildings were planned, most of them being fortified homesteads of a type already well known in Tripolitania. The sloping reinforcement walls which surround these buildings testify to the earth tremors which shook eastern Libya during the later Roman period and caused the collapse of the great Doric temples of Cyrene.

The work of the expedition has thrown some new light on the Roman roads of Cyrenaica, and on the defensive system organized in the later Roman period. The Roman frontier organization seems to have been less deep than in Tripolitania, and was strongest in the south-western zone towards the Syrtica, from which direction the main barbarian attacks seem to have come. At Zaviet Msus, on the line of the desert route used so frequently during the recent war, a small Roman outpost was covered with the names of soldiers stationed there to protect the important cisterns and control the caravan routes.

Remains of the early Islamic period were also encountered by the expedition, including the site of the city of Sort (Medinet Sultan, near Sirte), and a line of mud-brick forts along the caravan route between El Abiar and Mechili. The last-named were probably intended to protect the important desert track from Egypt to Tunisia, which passed by Mechili, Zaviet Msus, and Agedabia.

The expedition has been working under the auspices of the Map of Roman Libya Committee, which was set up last year under the chairmanship of Professor R. E. M. Wheeler. Generous contributions towards the cost were made by the Royal Geographical Society, the University of Durham, the British Academy, the Society of Antiquaries, the Oxford and Cambridge Craven Committees, and other bodies. Graduates and undergraduates of Oxford, Cambridge, Durham, and Reading universities took part in the field work under the direction of Mr R. G. Goodchild.

The results show that much remains to be done in the recording of ancient buildings in Libya, but that the geographical limits of ancient occupation are fairly clearly defined. A complete archaeological survey would require many years of field-work, but useful topographical information can be obtained in a few seasons of long-range reconnaissance. During its two-month survey this summer the expedition covered nearly 1,500 miles of terrain.

THE CITY WALL OF KANO

The climate and early type of civilization developed in Nigeria did not aid the preservation of ancient monuments, but arid conditions as found in the Northern Provinces, which approximate closely to those of Upper Egypt, have dealt more kindly with the early efforts of Man than have the damp, humid jungle conditions found over the rest of the country. The only important buildings of antiquity in Nigeria which can be immediately referred back to early civilizations are the ancient walls round the large cities of the Hausa States in the Northern Provinces. Of these, the wall surrounding Kano is probably the oldest and certainly the most important one. The gaps in its history are wide indeed, but one can obtain quite a fair picture of its origins and development from contemporary writings, although other records from the area have not been preserved.

According to tradition, Hausa peoples approximately 1000 years ago migrated from Daura (N. of Kano) under their leader Kano and founded the town bearing his name. It is assumed that the wall was built at the same time. Being an important trading and transport centre, which it still is to this day, Kano was always a prize for conquering tribes and nations, consequently its wall would be preserved and kept in good repair for military reasons by all conquerors. The city was kept as a vassal under varying rulers for centuries, until in 1805 it came under the Fulani Empire and was the capital of Kano Emirate.

Although there were many rumours concerning a large and important walled city of Kano, it was not until 1824 that Clapperton actually visited it: the first time a European had entered its gates. He gives a brief description of the wall as being oval in shape, made of clay, 30 feet high, and approximately 15 miles in circumference. A dry ditch ran round the wall, both inside and outside, thus increasing its military value; 15 gates, including one recently built up, gave entrance to the city. The entrances were opened and closed daily and could be defended in case of attack. The next visitor to Kano was Dr H. Barth in 1850, who found traces of an older, dilapidated wall inside the city itself. This wall was not complete but may well have been the original site of the early township before it expanded to its present size. From that time, Kano was no longer a city of mystery but open to free trade with other Europeans.

During the British conquest of Nigeria a punitive column of British troops in 1903 attacked the main gates of Kano, but the attack failed. A neighbouring gate (Nassarawa gate) was breached; the town was stormed with few casualties; the Emir's palace was occupied and order quickly restored. Kano then came under the administration of the British Government.

The present state of the wall is bad. No repairs seem to have been undertaken since the British conquest of the city except that certain entrances have been widened to allow the entry of motor traffic into the city proper. In parts the wall merely consists of a high mound of sun-hardened mud and the protective ditches are gradually filling in to ground level. The gates and platforms built for defence have been removed, and many entrances hollowed out to form leper-holes. There are still 15 gateways into the city. The old ones are too small to allow even a small car to drive through in comfort and the wall in their neighbourhood is usually in a better state of preservation than in the neighbourhood of the new entrances to the business quarters. All remains of the ancient wall noticed by Barth have completely disappeared.

Unless preservation is undertaken quickly one more irreplaceable link with early historical times will have been irreparably damaged.

P. B. Adamson.

Reviews

JEBEL MOYA. *By* FRANK ADDISON, F.S.A. *The Wellcome Excavations in the Sudan, Vols.* I *(text) and* II *(plates), published for the Trustees by the Oxford University Press,* 1949. *Price* £6 6*s*.

It seems that the gestation period of the greater excavators, like that of mammals, increases in proportion to the size of their excavations. Dr Reisner, who dug continuously for three decades, died before his first substantive publication appeared ; and Sir Henry Wellcome's excavations have had to wait for nearly forty years. Though covering four seasons only (1910–14) they were conducted on a colossal scale. The main purpose was philanthropic ; but as a business man well versed in scientific procedure, Sir Henry took care to have the work properly supervised, his archaeological adviser being Dr Reisner, whose method was adopted and some of whose trained Egyptians were employed. The site selected was a poor hill-village in a granite *jebel* in the Gezira, 20 miles west of Sennar on the Blue Nile where now is the Mekwar dam. It was not only a village but also a huge graveyard ; the huts, probably just grass *tukuls*, had left no trace behind, and the evidence consisted chiefly of what came from a few of the graves and the miscellaneous debris of the inhabitants. Thanks to the scale of operations so much was found that it is possible, for once, to trust negative evidence ; from which the first occupation appears to have begun not before 1000 B.C. and to have ended about 400 B.C. The culture was neolithic, with a few stray imports from the north Sudanese kingdom of Napata. The absence of Meroitic imports suggests that Jebel Moya was abandoned before the capital shifted southwards from Napata to Meroe near Shendi.

Sir Henry's selection of Jebel Moya was probably not primarily based on its archaeological importance (which was not much) but on other grounds. He was a benevolent autocrat, and liked to sit in his tent and direct the operations of his staff like a general. For security reasons the expedition's camp was run on military lines, complete with a Camp Commandant, armed guards and night watchmen perched on look-outs (and usually asleep). A megalithic building called the House of Boulders was built where he might reside. ' Uplift ' was the order of the day ; efforts were made to encourage thrift and sobriety amongst the long-suffering villagers, and an Order of the Peacock instituted for teetotallers. (The peacocks were imported from England and kept in the camp zoo). All who applied for work were taken on, and it was up to the staff to find work for them. As may be imagined, the archaeological work suffered, for the archaeologists (never more than three in number) were outstripped by the pace of the excavations and their records necessarily got behindhand. Nor were they allowed free hand. Their weekly diaries were submitted to the Chief, who returned them with criticisms. They were also bound by a very severe written contract.

Nevertheless, even if the results were disproportionate to the total expenditure—which cannot have been far short of £100,000 (of pre-1914 value)—when the final balance is drawn there is beyond all doubt a substantial profit to be recorded. This book is the chief item. It has been compiled by an experienced archaeologist who knows the Sudan but took no part in the excavations himself. Writing, as he says, as a detached observer, he has succeeded in producing a lucid and even readable account of the work. That is a really remarkable achievement, for in spite of Sir Henry's insistence on method—perhaps even because of his over-insistence on it—the materials with which Mr Addison had to deal were often imperfect and sometimes contradictory. Within those four years there were constant changes of method and of archaeologists, resulting in confusion. The finds passed through many vicissitudes; some have disappeared, including some of the best; labels were obliterated or altered. But so huge was the scale of operations that, even so, much was left to work on.

It must not be forgotten that when Sir Henry began to dig in 1900 modern methods of excavation had only just begun to be used in Africa and Asia, and that he adopted them. He was not a Pitt-Rivers (though there are some points of resemblance) nor could he even have claimed to be an archaeologist perhaps; but he had the root of the matter in him; and when he decided to excavate he turned for advice to the man who did for Egypt what Pitt-Rivers did for Britain, namely, Dr Reisner. This admirable book, to be followed (soon we hope) by another, is a fitting memorial, and his Trustees, in publishing it, have earned the gratitude of all archaeologists. O.G.S.C.

THE SARAWAK MUSEUM JOURNAL, issued by the Museum, Kuching, Sarawak; *May*, 1949 (*vol.* 5: NS. *no.* 1, OS. *no.* 16). *Annual Subscription, covering at least one issue yearly.* $6.00 (*Straits*).

It is always a good sign when a publication tells one all about itself, how to get it and such like, as this one does. The fact that this one is edited by Mr Tom Harrisson is a guarantee of vitality. The contents are a mixture of anthropology, archaeology and natural history; several of the articles are by Borneans, one in a Bornean language. Space forbids lengthy analysis; the editor's introduction strikes the right note, and we wish him every success in his work of introducing the techniques of western science to the east. (Could not the making of good half-tone blocks be added to them? There seems to be only one firm that can do so between Europe and the Pacific—and it did not make those in this Journal). An article by a Malay scholar on the art of Anyi, a Kelabit, reveals a live art and an artist who, besides the normal mediums, can work in others less durable. His formal patterns are most pleasing. Could he not be asked to design a new cover for the journal, in collaboration with the editor? There is a most interesting account, by Mr Jamuh, a Dyak, of the erection of large wooden poles which will be of interest to British prehistorians, who would as gladly read a description of the construction of Woodhenge by a Bronze Age Briton. There is much about languages, a remarkable tale of a snake that was seen to glide through the air for 250 feet, an account of recent Sarawak stamps, and notes on orchids. There is a judicious admixture of readability and specialism. The journal will be found valuable by all (including specialists) interested in both man and nature in the great island of Borneo; we are sure that it will prosper under its present editor and wish it every success. Both it and the subjects dealt with fully deserve official support. One minor point: in view of its bearing on human history, could not geology find a place in it? O.G.S.C.

THE OLD STONE AGE IN THE ANGLO-EGYPTIAN SUDAN. *Sudan Antiquities Service, Occasional Papers No.* 1. *Deighton Bell, Cambridge*, 1949.

Northwest of Khartoum, 1 kilometre below the junction of the Blue and White Niles, the Nile has on its left bank a deeply-cut torrent-bed called Khor Abu Anga, which widens out suddenly at about a kilometre from Omdurman, *i.e.* at two or three kilometres from the Nile. The bed studied by the author is situated, if I understand aright, immediately above this town, whose inhabitants exploit its deposits for the materials of their houses *en pisé*.

Resting upon (1) the Nubian sandstone series, with ferricrete layers and mudstone *passim*, one can distinguish the following strata : (2) decomposed Nubian sandstone, with bands of ironstone pebbles in place : (3) occasionally, iron-cemented conglomerate (max. thickness 5 feet) : (4) coarse gravel of ferricrete sandstone and indurated mudstone, sometimes as much as 3 feet thick : (5) fine ironstone gravel, often cemented with clay (average thickness one to two feet) : (6) in places, especially upstream of resistant barriers of layers 1 or 3, a layer of white calcareous sandy clay (with shells of *Limicolaria flammata*) probably deposited at the same time as the ' mesolithic ' of Khartoum [1].

There have been found *in situ* 185 worked stones; almost all of them artifacts of the ' Pre-Chellean ' and ' Chellean ' type (44 in layer 4 and 4 at the base of the next layer) ; ' Early Acheulean ' (15 in layer 4 and 6 in the deposits of layer 5 immediately above) ; Upper Acheulean (1 in layer 4, 21 in the next above it, where were also found 31 smaller hand-axes assigned to the ' Late Acheulean'). Lastly, from layer 6 have come a dozen hand-axes, one of which is thin, with carefully chipped base, belonging to a developed type ; an ovate with inverse flaking of slight extent and a ' rough ferricrete lancehead ' : a thick foliated hand-axe which the author regards as suggesting the presence of the Toumbian (' Sangoen '), various implements typical of which have been found on the surface or in gravels relaid at an epoch ' where the topography was beginning to assume an appearance similar to that of today '. This is in my opinion a rash hypothesis ; one could find similar specimens in any Acheulean deposit. With these surface or rearranged objects are found discoid cores (or those of the transverse type of Victoria West) and their flakes.

More than 90 per cent of these implements are of ' ferricrete sandstone ', the others being of ' silcrete sandstone ', except for some of ' indurated mudstone ', rhyolite, basalt or quartz.

Is the Toumbian then derived from the ' Late Acheulean ', or is it independent thereof ? Are certain foliate specimens the prototypes of the ' Solutrian-like artifacts ' of the Egyptian predynastic period ? Such are the questions which the author asks before proceeding to the examination of other sites to the south and as far to the north as Wadi Halfa and Faras. We may mention the site of Wadi Afu, 80 kilometres south of Khartoum, on the left bank of the White Nile, where a thin deposit of coarse gravel has yielded objects of the Upper Acheulean. Those found elsewhere, often *in situ*, are assigned to the ' Prechelleo-Acheulean ' (pebble-tools collected from gravel at 52 metres on the top of Jebel Nuri near the 4th cataract) ; and to the ' Chelleo-Acheulean ', with flakes of the ' block-on-block ' technique (Clactonian) from the 10-metre terrace of the Atbara above Sarsareib. A kind of ' Epi-Levalloisian ' with typical cores (Pl. 27, fig. 1), such as occur in the Aterian, have been found superficially in the same region. At Amara between the 2nd and 3rd cataracts, another surface site has yielded a complex series of implements amongst which one observes planes related to those of the Fayum Neolithic B. Other implements have been found far from the Nile (wells of Abu Tabari, 400 km. w. of the 6th cataract), which suggests a much damper climate than that of today. That was true also, as we know, of the period of the ' Mesolithic ' of Khartoum.

The discoveries at Abu Anga are important. They are a satisfactory sequel to that of Khartoum, and Mr A. J. Arkell, by these two publications, has notably advanced our knowledge of the prehistory of the eastern Sudan. The illustrations are fully adequate, but scientific publications should refrain from publishing figures without descriptive titles, enforcing a laborious search in the text and making the reading of it unnecessarily difficult.

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[1] See review in ANTIQUITY, September 1950, 151–4.

R. VAUFREY.

THE EARLY CHRISTIAN MONUMENTS OF WALES. *By* V. E. NASH-WILLIAMS. *pp.* XXIV+258, *with* 71 *plates and* 261 *illustrations in text. Cardiff: University of Wales Press*, 1950. *£*4 4*s*.

Early Welsh history from the end of Roman rule to the extinction of native independence in the Edwardian wars is a story that must be reconstructed from many different sources. Archaeology has a large contribution to make, but many of the antiquities which should illuminate this study have yet to be identified. One series is known and covers the whole period. The early Christian crosses and inscriptions start in the 5th century and continue, as this work shews, in an unbroken succession. A comprehensive inventory and survey has long been wanted. Dr Nash-Williams has now supplied this need and all students in this and allied fields will be grateful for the industry and scholarship, which are apparent on every page of this book.

The major part of the volume consists of an inventory, arranged alphabetically under counties and comprising 415 monuments. The descriptions are clear and concise and the comments, iconographical and historical, are factual and authoritative. The selection of references is not always consistent. Normally only those publications which make a substantive contribution to the study of the monument are cited. A rather fuller bibliography would in many cases have been convenient but the student can always refer to the author's earlier list in the *Bulletin of the Board of Celtic Studies* (vol. VIII, 1936) for a reference to the sources in which the early vicissitudes of the monuments and similar questions may be examined.

Very few omissions and corrections need to be noted. Removal for their better preservation is a continual process and it is too much to hope that any list will be fully up to date in its record of the present position of these monuments. The crosses from Erwhelm (no. 47) and Neuadd Siarman (no. 65) are now in Brecon Museum; the inscribed stone from Llanol (no. 6) is in Bangor Museum. More important, the inscribed stone from Llanfaelog (no. 10) has been found in use as a lintel in the barn at Pensieri bach. Incidentally this inscription can now be seen to read MAILISI with a horizontal final I as shewn in the original drawing. It might be added that both Eliseg's Pillar (no. 182) and the Maen Achwyffan (no. 190) are now under the guardianship of the Ministry of Works, a destination which might well be considered in the case of other important monuments which it is desirable to preserve *in situ*. Under no. 183 for Holyhead railway read *road*; under no. 390 for St. Dogmaels read *St. Dogwells*. The following monuments should be added to the inventory:—Caernarvonshire: the font at Pistyll with debased plaitwork and the cross-decorated boulder from Glan Beuno on the Seiont, now in the Eglwys y Bedd at Clynnogfawr. Pembrokeshire: the rock-cut cross on the cliff west of the church at Nevern. The boulder now at Clynnogfawr has a carefully cut cross. The tradition connecting it with St. Beuno is probably trustworthy and it thus provides a welcome confirmation of the early date proposed for the oldest stones in group II. Finally it should be noted that in the case of group IV (Transitional Romanesque Monuments, 11th–13th century) the inventory is less comprehensive than in the earlier periods. To mention only Meirionydd: there is no record of the inscriptions at Llandecwyn and Llanelltyd, which certainly fall within this classification.

Dr Nash-Williams' introduction arranges the monuments in four groups: simple nscribed stones (5th–7th century), cross-decorated stones (7th–9th century), sculptured crosses and cross-slabs (9th–11th century) and transitional Romanesque monuments 11th–13th century). The second group continued in use locally beyond the end of the period indicated and the author lists some 20 examples which he considers to be survivals. This list should probably be extended. In particular the more elaborately designed crosses (e.g. fig. 5, nos. 9, 29 and 36) seem likely to be contemporary with group III.

There are also valuable appendices analyzing the letter-forms and the decorative patterns and representational motifs. The patterns, both in this appendix and in the text, are described by reference to Romilly Allen's well known classification. The long and excellent bibliography includes Macalister Corpus Inscriptionum Insularum Celticarum without reference to the second volume (1949), which presumably appeared too late to be used. The book is illustrated with an admirable and carefully selected series of photographs and drawings. Many of the 71 plates shew as many as 6 or 8 of the simpler, smaller monuments. Some, like the writer, will regret that so many of the nscriptions have the letters outlined in chalk. This practice facilitates the quick reading of the lettering, but aesthetically it detracts from the appearance of the monument and scientifically it makes the checking of a doubtful letter more difficult. This is a minor point and this notice must not end on a note of criticism. Dr Nash-Williams has carried out a magnificent piece of work and the University of Wales Press has attained the highest standards of production. C.A.R.R.

CULTURE IN EARLY ANGLO-SAXON ENGLAND. *By* D. ELIZABETH MARTIN-CLARKE. *Baltimore, Johns Hopkins University Press*, 1947. (*London: Geoffrey Cumberlege*). *pp.* 100, *pl.* XXVIII. *Price* 12*s* 6*d net.*

Mrs Martin-Clarke has long taught closer collaboration between literature and archaeology in the field of Anglo-Saxon Studies. This attractively produced book published in America, and being a set of lectures delivered by the author at the Johns Hopkins University in 1945) is the printed expression of this creed. Mrs Martin-Clarke uses the archaeology of the period as a general framework to what she writes. We read in the preface that 'the subject-matter is for the general public interested in early culture'. In fact the book forms a handy introduction to aspects of early Saxon literature, and brings home successfully its general relationship with the archaeological material. The literary matter will be of interest to students of archaeology not familiar with the literary background, and *vice-versa*. On the archaeological side however, which predominates, the book does not advance far beyond Stjerna and Baldwin Brown. The only new matter that obtrudes itself is the Sutton Hoo material, which is continually referred to. In 1945 the author was still necessarily dependent almost entirely on the 1940 accounts of the find, but the fact remains that a great deal of Mrs Martin-Clarke's Sutton Hoo material has inevitably been superseded, and some of it is unsound. Mrs Martin-Clarke does well to emphasize the visual approach to early literature. 'Archaeology forces the student to use his eyes'. The visualization of objects 'makes the students more alert to the symbolism which is the basis of all language'. It is all the more unfortunate that the selection of illustrations and the use made of them does not live up to this promise. The interplay between text and pictures is not as happy or illuminating as it could have been, nor has enough care gone into the correlation of the two. To quote only two rather obvious instances, there would have been no justification even as far back as 1940 for the version of the Sutton Hoo purse that appears as Plate XXII, a pathethic illustration of the corresponding text (p. 90).

And why is not Plate XVIII (captioned 'Viking Camp at Trelleborg'), explained somewhere for the uninitiated for whom the book is meant? To the archaeological eye not familiar with the details of the unique Trelleborg plan it looks like a close-up of patches on the bottom of a bronze bowl that has somehow attracted the wrong caption. To the literary student and general reader it will probably be meaningless. Perhaps some of these short-comings in the production of the book should be put down to the difficulties of transatlantic liaison between author and publisher, but they are none the less unfortunate. In general, there is a lack of precision in wording and a looseness in description of archaeological material that is apt to be misleading or difficult to grasp. The general level of accuracy seems disappointingly low, as far as archaeology is concerned.

R. L. S. BRUCE-MITFORD.

STUDIES IN ANCIENT GREEK SOCIETY, THE PREHISTORIC AEGEAN.

By GEORGE THOMSON. *Lawrence and Wishart Ltd.,* 81 *Chancery Lane, London, W.C.* 2, 1949. *pp.* 624 *with* 85 *figures and* 12 *maps.* 42*s.*

This volume is planned as the first of a series of studies in ancient Greek society and is devoted to a consideration of the prehistoric Aegean. Our knowledge of the prehistoric Aegean depends largely upon archaeology. Any scholar, therefore, who has not had the advantage of a first hand acquaintance with the archaeological evidence is rash in attempting such a study as this without expert guidance. The bibliography shows that Professor Thomson has consulted many books, but he seems as far as archaeology is concerned to rely on secondary rather than on primary sources. (1) In a study of this character the original reports of the excavators should always be regarded as the basic documents.

In his seventeen chapters (nearly six hundred pages of text) the author often fails to grasp the implications of the archaeological evidence and sometimes misquotes it. No great palace has been excavated at Boeotian Orchomenos (p. 193). It is hardly correct to say (p. 29) that Mycenaean princes 'did nothing to improve the technique of production'. (2) The whole development of culture in the Late Helladic, or so-called Mycenaean, period shows that there was a steady advance in technique in most crafts, notably in architecture, pottery, and metal working. Schliemann did not discover at Mycenae 'mostly in tombs' (p. 239) a large quantity of female terracotta figurines. Does 'statutory groups' on p. 182 mean groups of statues? In any case it is hard to see how they could confirm the legend that Amazons founded the sanctuary of Artemis at Ephesus. Burial in tholos tombs was not a standard Minoan method of interment and the so-called tholoi of Mesara of the Early Minoan period can never have been tholoi (p. 249). On the same page it is stated that 'one of the sepulchres in the Grave Circle at Mycenae was in continuous use for two centuries (1450–1250 B.C.) and a family likeness has been recognized in the skeletons'. The skeletal material of the Grave Circle at Mycenae, which is very slender, has never been scientifically examined and the graves themselves date, as the author himself says on p. 371, from the 16th century B.C.

The author has studied Greek legend and mythology profoundly and has also read much comparative anthropology, but his results are most speculative and cannot be regarded as proved. He mixes archaeology and legend, as in his suggestion that the neolithic users of Dimeni ware in Thessaly were Lapiths and introduced Greek into prehistoric Greece. From this he further deduces that Poseidon at Corinth is a Lapith (pp. 184, 265). On page 383 we read 'Danae's subterranean prison seems to be a faint memory of the Shaft Graves, confused with the custom of secluding girls at puberty'.

With many of the author's points such as the evidences for matriarchy and for the prehistoric origins of classical Greek religion and belief no one can quarrel and he often refers to anthropological parallels with good effect. His method of argument, however, i unsafe, for he is apt to confuse the factual and hypothetical and he treats all legends and myths as if they were of equal value. Indeed by using such methods an ingenious writer could prove almost anything. The fifth section of the book, on Homer, is the best because here his essential scholarship and knowledge of ancient literature have full play unencumbered by mythological speculations and archaeological misunderstandings. The Homeric student will find in it much sound sense and pointed criticism as well as a genuine feeling for the beauties of Greek literature. A.J.B.W.

LOUGH GUR EXCAVATIONS. 'Three Marshland Habitation Sites', *by* S. P. O'RIORDAIN; 'Two Barrows at Ballingoola', *by* MAIRE MACDERMOTT (*J.R.S.A.I.*, 1949, 126–145—*not sold separately*). 'Carraig Aille and the "Spectacles"', *by* S. P. O'RIORDAIN (*P.R.I.A.*, LII, C, *no.* 3—15*s*).

These reports are an addition to the already substantial record of Prof. O'Riordain's work at Loch Gur and to our knowledge of house types. The structures recorded are three houses or huts (Grange and Ballingoola III and IV), the first at least dated not earlier than the Iron Age; two ring-barrows (Ballingoola I and II) of the Rathjordan type, perhaps not much later than a scatter of neolithic and beaker sherds found beneath them; two stone forts (Carraig Aille I and II) with associated buildings, dated to the 8th to 11th centuries A.D.; and the similarly dated undefended habitation site of the Spectacles.

With such wealth of material selection is inevitable and attention will be directed here to the house types and the enclosures containing them. Commencing with the humbler marsh sites, Grange is shown to be a small enclosure with earth bank and ditch containing a round house 5 m. in diameter, placed to one side of it. The house was floored with clay, walled with wattle and clay and provided with a central hearth and another hearth outside the door, while the clay floor extended outside the house and served as a working area. Some timber was laid down inside the entrance to the enclosure where the animals churned up the turf. This is a typical peasant homestead; the house was no doubt used mainly for sleeping and storage and, as Prof. O'Riordain wisely comments, life was lived largely out of doors.

Ballingoola III and IV were two contiguous wattle and clay huts some 6 m. in diameter without any surrounding enclosure. Prof. O'Riordain believes that the walls were banked externally with clay and draws a comparison with Dr Bersu's externally-banked farmyard wall at Ballacagan in Man, but such simple structures could arise independently. Their most interesting features are their shallow ditches immediately outside the line of the wall. Such have been found by Mr Stevenson in Scotland and it would be useful to know how far these are mere quarry ditches and how far they served as 'dry areas' to carry off the rainwater from the roof; calculation might show whether the excavated material was in excess of the quantity required for the former purpose. Prof. O'Riordain suggests use of the two huts as herdsmen's shelters, but the finds hardly support even this degree of habitation and, while one has an unenclosed central hearth three times remade, there are no external hearths or working places. These facts, and the absence of any enclosure, suggest rather that the buildings were used for storage or animals.

In contrast to these two elementary structures, which might be built in any age, the Dark Age sites were of much more pretentious character and yielded a wealth of occupation material, which will be invaluable for comparison with contemporary sites in Ireland and Western Britain. Of the three, Carraig Aille I and II, perched uncomfortably on a ridge, seem to be defensive in origin, though houses gradually spread over and beyond the wall of II. Each had a well-faced rubble wall 11 to 14 feet thick and provided with steps to reach the wall-head, a feature which is not necessarily defensive. The maximum wall height is now 3 feet 6 inches but is thought originally to have been 6 to 8 feet, and it might be possible—and would be valuable—to test this by measuring accurately the quantity of debris lying along a 10 foot length of wall. The areas enclosed were small, the average internal diameter not differing much from 100 feet in either fort, and originally the arrangements for shelter within them seem to have been of the most transitory. From this fact, from their site and from the enclosure external to I, which probably served for cattle, they are likely to have served as places of refuge in tribal warfare.

Despite its site, however, Carraig Aille II ceased to be such ; a gap was made in its wall and a small but wealthy suburb grew up outside. Here, and in the undefended Spectacles site, more or less round buildings occur, but the tendency was to the rectangular. An interesting type was a walled rectangular courtyard covered over at one end ; the house portion being divided from the yard by a wooden partition. Wood was extensively used in construction. The narrow walls of faced rubble would have stood to little height unless supported internally and externally by a wooden framing which is revealed by postholes cut in the rock. Sometimes earth replaced stone as a filling. Prof. O'Riordain has here identified a type of building, related to that found by Dr Bersu in Carnarvonshire and still surviving in the Faeroes, which is in essence a timber-framed one. It is to be suspected that many of the slight walls surviving as a ruin of stone or an earth bank in our upland areas were similarly timber-framed, and are to be contrasted with the thick-walled blackhouse of the Hebrides, where wood could be found only for rafters.

Prof. O'Riordain is to be congratulated on recovering so much of these ruined structures. Working with student labour, and by the method of excavating in rectangles divided by baulks (subsequently removed) which served as barrow-ways, something was necessarily sacrificed in the highly complex stratification of uneven rock floors, and the classification of the great range of articles of adornment and domestic use is based mainly on comparison with already known material. The finds are excellently illustrated with line drawings, and a few less successful photographs, and the student of the Dark Ages in the West is greatly indebted to Prof. O'Riordain for the heavy work entailed in their examination and description. L.S.

THE WILBOUR PAPYRUS. *By* ALAN H. GARDINER. I. *Plates* (1941), 73 pls. in *fol.* II. *Commentary* (1948), 216 *pp. with* 3 *tables*, 2 *maps.* III. *Translation* (1948), 138 *pp. Oxford University Press.*

The Wilbour Papyrus in possession of the Brooklyn Mus., U.S.A., belongs with a length of 10 m. to the most voluminous Egyptian manuscripts. Written in a very cursive Hieratic, its main part contains an account of measurements and assessments of land made over a period of about twenty-three days in the fourth year of Ramses V, i.e. about 1150 B.C. and 10 years later than the famous Papyrus Harris I of the British Museum with the report on all the donations of Ramses III to the Egyptian temples. The second part of the Wilbour consists of schedules concerning assessments of special fields of the Crown (khato-land of the Pharaoh) all situated in a Middle Egyptian region of 140 km. in length north of Minieh up to the Fayûm in the north. Apart from Upper Egyptian temples, too, the large Theban temples up to Hermonthis (south of Luxor) appear as land-owners, but no Lower Egyptian temples, apart from Memphis and Heliopolis are mentioned. Tax-lists offer no attractive reading, the more as they are composed in special terms concerning the position, owner, kind of soil, measures for experts. Their evaluation for the history of administration requires much patience and sagacity. Dr Gardiner has spent many years' work on it: in his *Ancient Egyptian Onomastica* (1947) he dealt with all the geographical schedules of Egypt. In order to clear up the technique of seizing and delivering he collected relevant documents in the 'Ramesside Administrative Documents' (1948) and interpreted them in *JEA.* 27. Having done this the editing of the Wilbour Papyrus became a pioneer's work, which in its learned commentary transgressed by far the scope of an *editio princeps* and will in future form the cornerstone for our knowledge of the agricultural administration in Egypt at a time when the state showed all signs of decay by fasting-strikes, riots and robbery.

As an interesting historical example taken out of the schedules of property I should like to quote the evidence about the real property of a royal harem in the town of Miwêr (Moïris) at the entrance of the Fayûm, a main region for catching fish and birds for court, where already in the XIIIth Dynasty a royal harem existed where Teje, mother of Echnaton, had her widow's seat (Med. Gurob) and where probably the Hittitic wife of Ramses II resided. Apart from explanations on questions concerning topography and cult, which can be illustrated by maps and tables, on metrology and other matters, the main question which the Wilbour Papyrus had not answered was: Did the temples retain the taxes or did they pay them to the Crown? Following a hypothesis of Černy, Gardiner decides for the latter solution, because it even happens that temple-property is administered as khato-land of the Pharaoh! Thus temple-land could be seized under certain circumstances (bad cultivation?). The anomaly contained therein, that the granaries of the residence remained empty in spite of exact assessments, is explained by Gardiner by pointing to the exploitation of the state by the family of the almighty high priest of Amun Ramsesnacht: one of his sons Usimarênacht, as steward of Amun, was not only one of the great cultivators of the crown-land in these districts situated at a great distance from Thebes (the second place is held by a standard-bearer of the Residence), but he was perhaps also the chief tax-master of the whole country.

I should like to point out another feature which the Wilbour Papyrus reveals: the division of the land among innumerable small-holders of all classes who cultivated the soil at a personal charge. Such small-holder properties in combination with a slow, red-tape administration mean a certain loss for the state in the due delivery to the central granaries. With the edition of the Wilbour Papyrus, in respect of printing technique a first-class work of the Oxford University Press, Sir Alan has given a rich present to Egyptology itself just before his 70th anniversary. HERMANN A. J. KEES.

SEVEN PRIVATE TOMBS AT KURNAH. By N. DE GARIS DAVIES. (Mond Excavations at Thebes II) *By* ALAN H. GARDINER. x, 59 *p.*, 41 *pl.* (4 *in colours*). *London: Egypt Exploration Society*, 1948.

To commemorate the many years' work of the late Sir Robert Mond for the tombs of the Theban necropolis Lady Mond has made possible the publication of this volume. Dr Gardiner has looked over the descriptions left by the late N. de Garis Davies (who died in 1941) and has especially examined the translations of the texts. In the course of the excavations made by Mond most of the seven tombs were uncovered or cleared. They are situated near the Ramose-Tomb (No. 55), published by Davies in 1941, but, in contrast with it, they do not belong to the persons of his rank of the necropolis.

If we except the decoration of the tomb of a steward of the high priest of Amun Meri in the time of Amenophis II (No. 45), partly preserved at a usurpation in Ramesside time, they originate from Ramesside time and do not show any more the richly coloured vivacity of e.g. the two pre-Ramesside tombs (No. 51,217) formerly published by Davies in his *Two Ramesside Tombs* (1927). Scenes taken from life, e.g. representing the deceased in the society of ladies, in the act of fishing and bird-catching such as are to be found in the tomb of the high priest of the Month of Hermonthis Hatiay (No. 324) are rare. They still recall the composition and motives of the Amarna age. Subjects taken from the sphere of ceremony and cult predominate there: among them the representation of a festival procession of the holy barge of the Month of Hermonthis when visiting the subsidiary temple of El Tôd and the departure of the statue of Thutmosis III, where the tomb-owner was a high priest (in the tomb of Chons called To of the time of Ramses II) (No. 31). Moreover the tombs offer many interesting subjects for the genealogies of the priests of the Ramesside time, e.g., that high priest represents two viziers as his ancestors and the formerly quoted Chons claims the same vizier Usermonth as his relative! All this is a characteristic reaction as to the denying of tradition in the Amarna time. It is with gratitude that we think of the late N. de Garis Davies whose skilled hand preserved for us so much of the charm of the Theban tombs.

HERMANN A. J. KEES.

SIALK, GIYAN, HISSAR AND THE INDO-IRANIAN CONNECTION. *By* D. H. GORDON. *Man in India*, XXVII (1947), *no.* 3, 195–241.

In this long and necessarily complex paper, Col. Gordon has made a valiant effort to correlate the stratigraphy and chronology of the three main excavated sites of prehistoric Persia, and to bring them into relationship with those of north-western India. To some extent his survey covers ground partially gone over by Dr Donald McCown and the present writer, while since the publication of the paper, Dr Claude Schaeffer's monumental study of Western Asiatic stratigraphy and chronology has appeared.

Gordon's main contention is the necessity of a short chronology for the Persian material after Early Dynastic times, and in his scheme Hissar III is dated 2100–1100, with its final phase overlapping with Giyan I and Sialk VI (the ' Necropole B '). Now some overlap between the last two periods mentioned seems certain enough. Schaeffer, discussing the matter in great detail and making certain important corrections in the dating of the cylinder seals and scarabs from the B Cemetery, would date the A Cemetery (Sialk V) at 1400–1200, and the B tombs (VI) as 1250–1100 (higher dates by a couple of centuries than those originally proposed by Ghirshman). Schaeffer would conflate Giyan I and II and assign them to a period *c.* 1550–1200—a considerable reduction on Ghirshman's dates of *c.* 1800 for the beginning of Giyan II—Gordon would indeed set it back to 2000.

Both McCown and Schaeffer have found it necessary to assume a break between the strata of Giyan V and IV (the Giyan stratigraphy is numbered from top to bottom, contrary to that of Hissar or Sialk). The former would end Giyan V in mid-Uruk times, and begin IV ' not later than Early Dynastic III ' (*Comp. Strat. Early Iran*, 48)—say about 2500. The latter, not concerned with the date of Giyan V, which is outside the chronological limits of his study, begins IV/III at 2100. Gordon however would prefer to bring down the end of V to about Early Dynastic I times, and to make no chronological break between V and IV. Although McCown feels there was ' unquestionably a long gap between the end of V and the beginning of IV ' (*ibid*, 48), and Schaeffer in his *Table Synoptique IX* indicates such a gap, of at least three centuries, yet in the text he is more moderate, and inclines to ' une rupture stratigraphique [implied by the use of the site as a cemetery] accompagnée probablement d'un hiatus ' (*Strat. Comp.*, p. 464). As Gordon says, McCown's case for a long hiatus is not unassailable.

It is worth while remarking, in parenthesis, that such differences of opinion are made possible only by the wholly inadequate recording of stratification in the Tepe Giyan excavations. To those of us trained in European archaeology the normal standards of Oriental excavation and publication come as a severe shock when we turn for information to the published reports. It seems to us astonishing that such a ludicrous travesty of a stratified section such as that given in the Giyan report (Pl. 3) could be published in 1935, and this is unfortunately all too typical of the products of the expeditions sponsored by many authorities in Western Asia. The ' section ' of Turang Tepe A published in the report of the American excavations in 1932 is even less adequate than that of Giyan, and of the stratigraphy of Turang Tepe C, Schaeffer ruefully remarks ' nous n'avons pas pu obtenir de coupe ' (*Strat. Comp.* p. 452). It is perhaps easy to criticize—though for evidence that European standards *can* be applied one has only to turn to the volumes of *Ancient India* published since the war. But in general we look in vain for interpretation and record of detailed stratification in the manner of Bersu, van Giffen or even of Pitt-Rivers half-a-century ago.

So far Gordon's contentions do not involve, on the whole, very serious discrepancies from (for instance) Schaeffer's chronology in general terms. But in his dating for the three phases of Hissar III he attempts a scaling-down for the end of the period which is far below any limits hitherto set. McCown in his classic work of 1942 set out to demonstrate a long chronology in which the Hissar III period ran from Early Dynastic I to Akkadian (i.e. about 2800–2300 on the current short chronology of Mesopotamia). I felt the difficulties inherent in this view when I was trying to correlate the Indian material with the Persian in 1943, and suggested making Hissar III ' not earlier than Akkadian and probably some centuries later ' (ANTIQUITY, XVII (1943), 181). But by 1947 I saw my chronology was too short, but did not feel ' that a date earlier than *c.* 2500 for the beginning is really defensible, and would prefer to bring it much nearer 2000 ' (*Ancient India*, No. 4 (1947–48), 32). A discussion with McCown early in 1948 ended in our agreeing on a terminal date for Hissar III of 2000, and the publication of Schaeffer's book later in the year, in which he gave dates of 2300–2000 for Hissar III, gave welcome conformation of our views. Gordon's dates of 2100–1100 do not seem to me to be at all admissible.

The evidence has been admirably marshalled and discussed by Schaeffer. The spiral-headed and animal-headed pins and wands (surely not all unfinished pins as Schaeffer would have them—what about kohl-rods ?) link Hissar III with the Caucasus, Anatolia and Luristan ; the mid-rib, hook-tanged daggers and spear-heads have good parallels at Ras Shamra, Cyprus, Tarsus and elsewhere, and like the axe-adzes are of ultimately Early Dynastic types, but the associations in the sites mentioned all converge to indicate a date round about 2200–2000. Schaeffer makes a significant grouping of the rich tombs of Maikop, Alaca Hoyuk, Turang Tepe (' The Treasure of Asterabad ') and others, with the Hissar III material in a similar chronological context. Historically this sudden acquisition of wealth and of Sumerian metallurgical techniques by the outer barbarians about the time of the temporary collapse on the death of Naram-sin, and the inroads of the raiders, seems apt enough. The dating of Hissar III as 2300–2000 seems to me to fit in admirably not only with the evidence from the Aegean, Anatolia and the Levant discussed by Schaeffer, but with the Indian evidence as well.

Gordon is brave enough to discuss the Aryan invasion of India in its archaeological context. Like the Druids in Europe, these Indo-European people have become so suspect owing to the nonsense written about them by fools (and, with the Aryans, by knaves as well), that respectable archaeologists hardly dare mention them. But after all, they did exist, and constitute a problem of first-class importance in the archaeology of Europe and Western Asia. Gordon summarizes the pieces of evidence pointing to movements of peoples eastwards into Baluchistan and India early in the second millennium B.C. (the axe-adze from Mohenjo-daro and other copper tools and weapons) and the Shahi-tump and Jhukar stamp-seals and shaft-hole axes point in the same direction. But he also draws attention to a first-rate piece of evidence, hitherto unrecognized, showing that people related to the Sialk Cemetery B folk were living in North Baluchistan, presumably about 1000 B.C. Cairns in a small cemetery at Moghul-ghundai dug by Stein produced a whole array of bronze objects—bells, rings, arrowheads, a bangle and a tripod jar—absolutely typical of the Sialk B tombs. To this may be added the arrowheads and grey-ware spouted pot fragments of similar affinities from Nad-i-Ali in Afghan Sistan, and, as I think we may now recognize, much of the pottery from the cemetries of Zangian and Jiwanri in the Makran published by Stein (vessels comparable with the Sialk *cruches à bec aplati* for instance).

And finally, recent field-work in Baluchistan by Miss Beatrice de Cardi has produced even more convincing evidence of such connections. From a site at Londo, in Baghwana, north-west of Nal (from which Stein had picked up a few sherds) Miss de Cardi was able to obtain abundant material comparable in many ways to the Jiwanri style of painting, and two sherds of exceptional interest. One, with a design in black and white paint on a red background, has a pair of addorsed animals with long horns but otherwise equine features and bristly manes very similar in style to many of the Sialk B animals. The other (black-on-red) has part of a frieze of horses in exactly the manner of the animals (horses and others) on the famous long-spouted jugs from the Sialk Cemetery B: the Londo sherd is a provincial version of the fine Sialk pottery, but the stylistic affinities are quite clear.

It looks therefore as though contacts between India and the west were either continuous for nearly a millennium after 2000 B.C., or, more probably, that there were two main movements of peoples from Northern Persia towards India, one (Shahi-tump, Jhukar, etc.) fairly soon after 2000 B.C. (the Indian evidence implies that it cannot be earlier) and the other (Moghul-ghundai, Jiwanri, Londo) round about 1100 or later. In one or both of these the Aryans must be involved, and elusive though these people have been in the past, archaeologically considered, I feel that it will not be very long before more definite evidence of their migrations comes to light. STUART PIGGOTT.

(NOTE: The Londo finds are referred to by the courtesy of Miss de Cardi and of the Director of the Institute of Archaeology, University of London, where the material is now housed).

GLASS THROUGH THE AGES. *By* E. BARRINGTON HAYNES. 240 *pp.*, 64 *plates in photogravure. Penguin Books*, 1948. 2*s.*

Whoever attempts the story of glass in so short a compass must concentrate on a few of the highlights. Mr Haynes has chosen two. The first part of the book sketches the history of glass from its beginnings in Dynastic Egypt to the acme of the unmechanized industry in the 18th century and lays most emphasis on the essential unity of that development; he carries us rapidly back and forth between east and west at different eras and shows how there was never any appreciable dissociation of Levantine and western workers, however thin the connecting threads may have become in Dark Age times. In his second part he chooses a portion of the subject for exhaustive treatment

and provides a full classification of English 18th century glasses. Written mainly from the collectors' standpoint, Mr Haynes's book will nevertheless appeal equally to archaeologists and art-historians, and the copious illustrations, which stress the ordinary rather than the extraordinary, are alone worth more than the modest price of this Penguin book.
D. B. Harden.

FIVE THOUSAND YEARS OF PAKISTAN. *By* R. E. M. Wheeler. *pp.* 150, *21 plates and 19 figures. Royal India and Pakistan Society, London.* 1950. 31*s* 6*d*.

We are told in the preface that the purpose of this book is to present a brief sketch of the imposing material heritage of Pakistan ; it is however sub-titled ' an archaeological outline ', and, in view of the fact that it is Prof. Mortimer Wheeler who is the author, anyone acquiring this book will, I am sure, expect it to be a handbook of the archaeology of East and West Pakistan.

I believe that ten additional pages of text, two more maps and about five extra plates would have made a considerable difference. This is open to the criticism that yet more pages, maps and plates would have made it better still ; with this however I disagree. By these remarks I do not wish to convey that this is not a good book, but that with comparatively small additions it might have been a better one. Particularly welcome are the attempts to bring the past to life by narrative descriptions of the city of Mohenjo-daro, the invasion of the Aryans and the arrival of Alexander at Taxila, all of which are archaeologically sound and help to make the dry bones live.

The first ten chapters contain good, clear, concise accounts of the cultures and industries of the Pakistan–Persian borderlands, of the Harappa civilization, the coming of the Aryans and the Buddhist period. The chapter on Hindu temples and fortifications is very welcome, as these particular examples are relatively little known, and so also is the plan of northern Kafir Kot. This fortress had not however the strategic value of the city of Akra, seven miles south of Bannu, which is not mentioned. Some reference might have been made to the fact that there are sites of the early historic period at all the points where rivers flow from the Waziristan gorges, and that Akra, commanding the Kurram and the Tank Zam, was the most important of them all.

The Islamic material is well handled, and a number of buildings which have hitherto been overshadowed by those of Delhi and Agra are brought into the prominence they deserve ; that of East Pakistan however suffers from being intrinsically rather mediocre, and this part of the book is only relieved from dullness by occasional historical anecdotes. Prof. Wheeler very rightly stresses the various processes and techniques employed in the production of objects of everyday use, and concludes with a plea that the many humble links with the past, now either regarded as commonplace or disregarded altogether should be recorded and where possible preserved before they disappear and are forgotten.

One can only judge that Prof. Wheeler felt that it would not be right to use Sir Aurel Stein's unpublished material on Las Bela and Bahawalpur, but by its omission we are deprived both of part of the archaeological heritage of Pakistan and of the chance of some new light on the inter-relations of the Amri, Harappa and, if I may use my own designation, the Ravi cultures, all of which one gathers are represented in Bahawalpur. No mention is made of the microlithic finds in southern Sind and at Jamalgarhi, which help to link the early stone industries of the Rawalpindi and Campbellpur Districts, where a few micros have also been found, with the arrival of the peasant communities from Iran. The important iron age cairn burial people, whose distinctive pottery, along with their burial cairns and iron tools, is found from Hazar Mardi in the Rudbar tract of SE Iran to

Moghul Ghundai in the Zhob, should have been introduced as one of the few cultures in this area that can be plausibly attributed to the middle centuries of the 1st millennium B.C.

I feel that the rock engravings of Mandori and other Middle Indus sites and the rock paintings of Chargul should have received notice and the former linked with the little-known engravings, unfortunately unrecorded save by description, found by Air Surel Stein in Baluchistan. While dealing with the Buddhist period, though the importance of the Bala Hissar is again rightly stressed, Sar Dheri is not mentioned nor the terracotta figurines which are such a feature of the Yusufzai sites and of Taxila. Not enough credit is given to the Parthians for the spread of Hellenic ideas. Though it is true that they maintained a form of iron curtain, this was largely that they might retain their valuable position as middlemen on the land trade routes, and their influence must have been at least as much as that of the merchants of Alexandria, in that they set the fashion in the East even if the merchants produced the goods.

Some of the points mentioned and others not mentioned might well have occupied the ten additional pages. The two extra maps are needed to show those places in East and West Pakistan mentioned in the text, and a more ample and less select bibliography would have been of more value to the student than the list of Protected Monuments, which in any case will have little or no interest for the general reader.

All this does not detract from the fact that there is a very great deal of valuable information in this book most ably set out, and where material has been omitted it is I feel because our exact information about it is judged by the author to be too scanty. This may be right, but it deprives the picture of material, which, though it can only be sketched in lightly, does help to complete the design. D. H. GORDON.

HERDSMEN AND HERMITS. *By* T. C. LETHBRIDGE, foreword *by* T. D. KENDRICK. *Cambridge* (Bowes and Bowes), 1950. 10*s* 6*d*.

The exchange of compliments between Mr Tom Kendrick and Mr Tom Lethbridge with which this book opens provides the most engaging of introductions. We soon after learn that the author is no academic archaeologist. He does not believe that humanity in the past, any more than in the present, can be studied from a library, or even from a motor road. He himself has farmed the lands and sailed the waters he is to talk of. He is in a very strong position.

He is a foe to jargon. Those persistent potters the 'beaker folk' are his *bêtes noires*, especially if qualified A, B or C. He shows from his Greenland excavations to what a level of unmeaningness a tribe can be reduced if judged by its one surviving artifact to the exclusion of its multifarious equipment in more perishable materials. He eschews 'impacts', 'impulses', 'influences': all those abstract terms which may mean anything or nothing in terms of the human beings which are the subject of archaeology, but so readily become subjects of discussion in themselves. For all this we can bless Mr Lethbridge, remembering that the *Decline and Fall* was due to his university not giving Gibbon a research scholarship and a lectureship to teach Greats students, but leaving him to make love to the future wife of Necker, to practise soldiering in Hampshire and to sit as a Trade Commissioner in Whitehall.

But Gibbon was at the same time a patient master of his facts, and here Mr Lethbridge is not always so strong. Accordingly his book falls into two parts of unequal value, the one dealing with those matters he knows from his own experiences, the other with those impressions he has gained from reading the works of others. It is good indeed to have these impressions, and the writers criticized will not mind that they are sometimes reproved for not saying what in fact they did say—Prof. Childe, for example, for *not* saying the Skara Brae community was a single great family. But we do not achieve 'a clear picture of the real Bronze Age in Scotland' by alleging that there are 'no traces of settlements with characteristic pottery and other relics', or of the cult of the dead among users of 'grooved ware' by ignoring the occurrence of their pottery in unurned cremation enclosures.

Let us turn to Mr Lethbridge's direct experiences. There is nothing better than his climb up the fowling cliffs of the Westman Island southwest of Iceland and his discovery, carved on the rock face, of a cross of a type known in the Scottish Islands. Whether this proves that the Westmen Islands were occupied by Celtic hermits before the Norse settlement, or, as the story in the Landnamabok rather suggests, by a Celtic community of escaped slaves, or whether it proves neither, the story has the immediacy of experience and is to be welcomed as such.

Valuable also is the observation that some Portuguese boats preserve the hull shape of Mediterranean 2nd millennium boats known to us from Egyptian grave models. Mr Lethbridge would derive the broken sheer of some Irish curraghs from this type, and he develops an enthusiasm for the deep-sea capabilities of the curragh which it is hard to share. One would feel happier about the 2nd millennium settlements in Western Scotland if wooden boats of the Egyptian type (five-part canoes) had been available, as tomb carvings extending at least as far west as Sardinia suggest they were. On the curraghs we can more safely listen to the sarcastic comment in a medieval Gaelic poem on a ship 'without oak timber' as 'all one ship of leather; she is not a ship complete for sea-going'.

In more scholarly vein is the suggested development of pins in Western Britain. In the scarcity of pottery the pin may prove a clue to Dark Age chronology and the discussion is valuable; a fuller account is promised in the required detail and we hope we shall not wait for this in vain.

Of such varied materials is the book composed. Not all will be equally valuable to remember; but this is not a book of reference, and we may be grateful for those lively and first-hand impressions which we shall not want to forget. L.S.

WHITHORN VOLUME. *Ed.* R. C. REID. *Transactions of the Dumfriesshire and Galloway Natural History and Antiquarian Society, Third Series, Vol.* XXVII, *Dumfries* 1950, 245 *pp. plus illustrations. No price.*

The society and its energetic editor deserve praise for the plan, conceived and executed, of devoting one annual volume almost entirely to articles on Ninian and Whithorn. Mr Ralegh Radford's preliminary report on the 1949 excavations at Whithorn raises the volume from local to national importance. A collection of essays on St. Ninian is bound to reveal inevitable clashes of opinion, and some contributions are highly speculative, if not imaginative. In two of the shorter articles photography and reproduction leave much to be desired. But the idea of a 'Whithorn Volume' is excellent; it deserves to be imitated and developed. F. T. WAINWRIGHT.

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